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MIND

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PSYCHOLOGY AND PHILOSOPHY

I.—THE PHILOSOPHY OF SAMUEL ALEXANDER (II.).

BY G. F. STOUT.

Enjoyment and Contemplation.

ALEXANDER draws a hard and fast distinction between the way in which we experience objects and the way in which we experience our own mental (*i.e.*, subjective) processes. We are said to *enjoy* the mental processes and to *contemplate* objects. Let no one suppose that this is only a new way of naming a distinction which can be readily understood and easily verified. At first I myself made this mistake. I took it to be in principle identical with the distinction between actual experience and thought. Then my main difficulty was with the view that only subjective processes and not sensa could be enjoyed, and also with the view that enjoyment and contemplation are mutually exclusive. But I soon discovered my mistake. The peculiarity of Alexander's view emerges with startling clearness in his denial that pleasure and pain are enjoyed. It is nonsense to say that the pain of a toothache or the pleasure of a lover in the presence of his beloved are not actually experienced. It is also nonsense to say that the pleasure and pain are existentially distinct and separable from the actual experiencing of them. Pleasure and pain exist only in being felt. When they are unfelt they not only cease to be known but altogether cease to be. I agree with Alexander that being pleased or pained is not the same as being

conscious of the pleasure or pain. There is always the difference between what is known and the knowing of it. But in this case not only the knowing or being conscious but what we know or are conscious of is actually experienced and only exists in being actually experienced.

Let us now quote Alexander's own account of what he means by enjoyment as distinct from contemplation:—

"Take the perception of a tree or a table. This situation consists of the act of the mind which is the perceiving; the object of which it is aware is so much of the thing called tree as is perceived . . .; and the togetherness or compresence which connects these two distinct existences into the total situation is called the experience. But the two terms are differently experienced. The one is experienced, that is, is present in the experience, as the act of experiencing, the other as that which is experienced. . . . The word 'of' indicates the relation between these two relatively distinct existences. The difference between the two ways in which the terms are experienced is expressed in language by the difference between the cognate and the objective accusative. I am aware of my awareness as I strike a stroke or wave a farewell. *My awareness and my being aware of it are identical.* I experience the tree as I strike a man or wave a flag." [1]

How, I ask, can my awareness and my being aware of it be identical? Such propositions are formally barred by Logic. What is known cannot be identical with its being known. Yet Alexander, when he speaks of awareness, means cognition. Everywhere he regards enjoyment as involving knowledge of what is enjoyed. Here he explicitly states that there is no difference between the enjoyment and the knowledge of it, between experiencing and the experience of the experiencing. What he says about the cognate accusative does not help him. For knowing is related to what is known not as striking to the stroke, but rather as striking to the object struck.[2] It is true that in the case of enjoyment the knowing is not existentially separate from what is known. But, as I have previously urged, the same is true of contemplation. Alexander's cognitive acts supposed to be existentially separate from the object cognised are impossible fictions. There are indeed what Alexander calls

[1] *S. T. and D.*, vol. i, pp. 11-12. Italics mine.

[2] I may point out that even the cognate accusative does not stand for just the same thing as its cognate verb. The verb, *e.g.*, *strikes*, refers to a process with successive stages; the noun, *e.g.*, the *stroke*, refers to the completion of the process, when the stroke has been struck.

conations, *i.e.*, ways in which the mind is interested in or occupied about the objects of which it is cognisant; and it may be that without such processes there could be no cognition. But in this respect there is no essential difference between contemplation and enjoyment. The enjoyed process consists in being interested in and occupied about its own object. But we may also be interested in and occupied about the enjoyed process itself as well as its object. In desiring food my attention may be wholly or almost wholly concentrated on the food as seen or smelled or thought of. But if satisfaction is postponed I become immediately interested in the desire itself. There follow exclamations such as: "I am dreadfully hungry; I would give the world for something to eat". Sometimes the interest in the enjoyed process is opposed to the interest in its object. This happens when we endeavour to suppress or conceal a desire such, for instance, as the morbid sexual cravings with which the psycho-analyst deals.

I conclude that "enjoyment" is not a way in which subjective states and processes are known, but a way in which they exist. I cannot see a fundamental difference between this way of existing and what I call being actually experienced. But I hold that sensa also are actually experienced. The question arises whether I am using the term with the same meaning when I apply it to sensa. If not, what is the difference? I have just pointed out that "enjoyment" of subjective processes is distinct from being cognisant of them. Is this also true of sensa as actually experienced? It seems too often assumed that the actual sensing of sensa consists only in knowing them by "direct acquaintance" instead of merely thinking of them. I cannot accept this view. What I find is that the actual experiencing is a necessary precondition of knowledge by acquaintance and is therefore not identical with it. It is no more identical with it than the placing of an object within my range of vision is identical with my seeing it. This is especially evident when the sensum is intensely pleasant or unpleasant or in other ways is in a high degree disturbing. Consider for instance a noise of overpowering intensity together with the organic sensa which accompany it. It is not my cognition of such sensa, however direct, but the sensa themselves which are so disturbing and intrusive. Their intrusiveness belongs to them as actually experienced and not as known. I cannot treat this question here with any approach to adequacy. I shall content myself with adding one argument which seems to me to have great force. If actual experience of sensa is just the same as knowing them by acquaintance it follows that nothing whatever can

be sensibly experienced without being thus known. Further, nothing can be true of actual experience which is not true of the knowledge by acquaintance which is supposed to be identical with it. Now whatever is actually experienced is actually experienced in its completely determinate particularity. We cannot actually experience shape in general but only an entirely specific and particular shape. We cannot be actually experiencing a whole without actually experiencing each and all of its parts in their particular detail, as I can think of the town of Sydney without thinking of each and all of its particular roads, streets, etc. But if the actual experiencing of sensa is simply identical with knowing them by acquaintance, the knowledge by acquaintance must be no less particular and determinate. There ought to be no subconscious sensa. Every detail which is sensibly experienced ought to be separately discerned. But I am fully convinced that this is not so. If I ask myself what I am sensibly experiencing at any moment I find that only a relatively small part is discerned in detail. I am cognisant of the rest only inasmuch as I am cognisant of the whole sensible experience to which they belong. If I try to analyse this whole into its component parts, I find that I can pick out detail after detail, yet can never make the analysis complete. Further, the transition is quite different from that to new sensa, as when after seeing black I see white which was previously unseen. As each detail emerges into distinctness I am aware of something that I sensibly experienced before I discerned it—became *conscious* of it. I cannot here repeat what I and others have said in support of this doctrine of subconscious sensa. Some persons reject it as inherently absurd. But I have always found that they are really begging the question. They begin by assuming that what I call actual experience must be identical with knowledge by acquaintance. On this assumption I, of course, agree with them that there can be no subconscious sensa. Fully convinced, as I am, that they really exist, I can only infer that the assumption is false.

Can we distinguish enjoyment from sensible experience by saying that what is enjoyed actually exists only when it is actually enjoyed, whereas sensa may actually exist when they are not actually experienced? Once we have dismissed the view that sensa are selections from or distortions of objective facts, I can discover no evidence that they do in fact so exist, and it is at least doubtful whether they could do so. I am myself strongly inclined to agree with Berkeley that they could not. But even if we suppose that sensa can and subjective processes cannot

actually exist without being actually experienced it does not follow that this is due to any fundamental difference in the way they are experienced. It may equally well be due to the difference between the nature of a subjective process and of a sensum.

Alexander, like James Ward, draws a hard and fast line between sensa and subjective process. He denies (though with some hesitation) that being pleased or pained by, with or about something is subjective or, as he would say, "mental". His reason is that pleasure and pain are sensory. It is curious that in this case at least he seems to have no way of directly determining what is mental. He has to fall back on this indirect criterion. To me it seems so clear that being pleased and the reverse are subjective that, if they are also sensory, I can only conclude that what is sensory constitutes or at least enters into the constitution of subjective states and processes. When we have got rid of the present alleged physical existence of sensa, this view does not present any insuperable difficulty. What stands in the way of it is a failure to distinguish the question of origin from the question of function. To be subjective is to be a way of being interested in something. Provided that this condition is fulfilled, a process is subjective whether or not it is partly or wholly sensory in its origin. In fact there seems to be good reason for asserting that subjective states are at least partly sensory in their origin.[1] I agree with Alexander that being pleased and pained essentially involve organic sensa with their feeling-tone. It is still clearer that the same is true of emotions such as fear and anger. Conation too normally involves experience of motor tension.

Knowledge of Other Minds.

In contrast with his usual thoroughness, Alexander's treatment of the way in which we know other minds strikes me as somewhat perfunctory. Generally he speaks of contemplation and enjoyment as if they were the only primary and direct "ways of apprehension". But when he comes expressly to deal with the knowledge one mind has of others he refuses to regard this as inferential. He adds it to contemplation and enjoyment as a third primary and direct way of knowing which he calls "assurance". He fully admits that knowledge of other minds *develops* through inference, just as knowledge of the physical world does. But in both cases the starting-point and precondition of the inferential process must be found in knowledge which is not inferential.

[1] I do not think that they are ever *wholly* sensory. But I have not time to discuss the question.

So far I can follow him and in general accept his arguments. In particular, I agree with him that "the idea of a foreign consciousness, unless directly supplied by some experience to that effect, is something to which we have no clue in ourselves".[1] But I cannot accept Alexander's account of the conditions under which this primary knowledge of other minds arises. According to him it arises in the contemplation of a special kind of object. If we ask what kind of object this is, he answers that it is the kind of object in which we are socially interested. Alexander gives as examples "parental or filial affection, or sexual love, competition in pursuit of prey, or jealousy".[2] Now the very meaning of the term social interest as Alexander understands it implies that it cannot exist apart from the idea or thought of the existence of other minds. But just for that reason it is putting the cart before the horse to say that social interest is an antecedent condition which first gives rise in the individual to the thought of minds other than his own. Unless this is already present, there can be only contemplation of physical objects. "There is," says Alexander, "all the difference between grasping a hand which returns the pressure and grasping an unresponsive piece of flesh in the shape of a hand."[3] This is true. But the difference would not be appreciated if in grasping the hand we did not apprehend it as belonging to someone who was, or at least might be, capable of responsive feelings and desires. If this condition were not fulfilled, the pressure of the hand grasped would not be apprehended as a social response. It would be perceived merely as a physical fact. The same difficulty which Alexander finds in the theory of inference by analogy applies also to his own view. Social interest presupposes the thought of mental life other than our own and cannot therefore account for its existence.

My own view[4] is that the thought of mind other than our own and connected with our own is primary and universal in the constitution of experience and in this respect is on a level with the thought of physical objects as involved in sense-perception. The function of actually experienced sensa in sense-perception is fulfilled by actually experienced subjective processes in the knowledge we have of mental life other than our own. This knowledge develops in detail through inference, in which

[1] *S. T. and D.*, vol. ii, p. 32.

[2] *Ibid.*, p. 33. Of course, this direct experience cannot be what I call "actual" experience but must be primary and non-inferential knowledge *through* experience. I had to argue this point in dealing with sense-perception. But here no argument is required.

[3] *Ibid.*, p. 34.

[4] Expounded in *Mind and Matter*.

the analogy of the bodily appearance and motions of others to our own, and also the experiences of social response, play a most important part. Of the two, social response is the more fundamental and important.[1] Where and as far as these special clues fail, our apprehension of mental life beyond our own becomes comparatively vague and indefinite. But there is no reason for regarding it as baseless or unimportant. The primitive tendency, however, is to assume an individual life more or less analogous to our own individual selves even where special evidence for it is lacking. This anthropomorphic tendency is gradually corrected with advancing experience in much the same way as are primitive illusions of sense-perception, and just as the correction of perceptual error presupposes that sense-perception is always concerned with the objective world, however mistaken we may be about it, I submit that the same holds for the apprehension of mind other than our own.

Space-time and Intuition.

Alexander holds that there is a way of apprehending which he calls "Intuition," coming under the general head of contemplation, but distinct from Sense-Perception. I have postponed consideration of this way of apprehending because I find it impossible to examine it without first taking account of what seems to me most untenable in Alexander's ontology. In treating of sense-perception this complication did not arise. For there Alexander's fundamental ontological position was one which I share myself. It simply consists in the assumption that there really is a spatial-temporal universe, existing independently of being perceived. I should also agree that our knowledge of this universe is primarily direct in the sense in which 'direct' is contrasted with 'inferential'.

But his doctrine of intuition as a distinct way of apprehending is founded on a peculiar ontological theory of the ultimate constitution of the spatial-temporal universe. The basis of this theory is the view of *pure* space-time as existing independently of and even prior to the things, processes and qualities which are ordinarily said to exist in space and occur in time. From this pure space-time everything else develops, or, as he also puts it, pure space-time is the "stuff" of which everything else is

[1] I hold Alexander to be wrong in regarding social response as the sole primary source of the thought of mind other than our own. But in all other ways what he says about it seems to me to be sound and admirably worked out.

made. It is as if he had said that pure number comes first and all countable things are evolved from it. Neither time nor space are anything at all apart from things and events which are not merely spatial and temporal but have other characters. Space is a universal order in which extended things and their parts are correlated and connected. Time is a universal order in which changes are correlated and connected.[1] The spatial order and the temporal mutually involve each other, and this fact is marked by the term space-time; and this, so far as I can see, is all that modern science means by it.

According to Alexander, pure space and pure time, considered apart from each other, are mere abstractions incapable of existing. But in their union as space-time they constitute one actual concrete process of great and ever-increasing complexity. In working out this view in detail he displays his own brilliance and ingenuity in a most impressive way. But what he says corresponds to nothing in my experience. It is all in the air, or rather in an airless region in which I at any rate find myself suffocated. I shall not, therefore, make any attempt to follow him in this part of his work.

What I am here especially concerned with is the correlation of pure space-time, as Alexander conceives it, with that peculiar way of apprehending it which he calls "Intuition". Pure space-time is according to him not perceptible by the senses either as a whole or in its parts. The proper objects of sense-perception consist in *qualities*, including under this head not only colours, sounds, smells, etc., but mass and energy[2] (what Alexander calls "materiality"). Pure unqualified space-time must therefore be apprehended otherwise than by sense-perception: this other way of apprehending is "Intuition". But intuition, though radically distinct from sense-perception, is none the less dependent on it. Apart from sense-perception intuition would be a mere faculty. The actual exercise of this faculty is indispensably conditioned by the perception of qualities. But this condition is also a limitation. We have no

[1] The correlation involved in measurement is of essential importance. Two extents are regarded as spatially equal which would coincide if they were superposed. Two changes are regarded as taking the same time which, if they began simultaneously, would end simultaneously. Relative changes of position, distance, simultaneity and succession, comparative speed and slowness, are perceived in perceiving things and events. We cannot perceive pure space or time or space-time.

[2] This usage seems to take away the distinctive meaning of the word. For mass and energy are quantitative and relational, not qualitative characters.

determinate apprehension of the particular parts of space-time which is not found in or derived from experience in which sense-perception accompanies and makes possible intuition. Intuition is, so to speak, in the dark except in so far as sense-perception strikes a light for it to see by. If it could be set free from this limiting condition, it would be an apprehension of the whole of space-time, including all its parts severally. "Compresence," in the wide sense attached to that word by Alexander, would be all that is required for cognition. We are ourselves, according to him, parts of space-time. All other parts are therefore compresent with us and would be known in all their detail, if intuition were not limited by sense-perception. Even with this limitation we are, according to Alexander, cognisant, not indeed of the whole of space-time, but of space-time as a whole.

This theory of intuition as a separate and independent faculty is essentially dependent on the ontological theory of pure space-time as a separate and independent existence from which qualities, including mass and energy, somehow "emerge". Holding as I do that pure space-time, apart from all that Alexander calls qualities, is an impossible abstraction incapable of existing by itself, I am bound to reject his doctrine of intuition as radically distinct from sense-perception. The reason why we cannot perceive pure space-time without qualities is that it can have no being without qualities any more than the qualities have any being apart from it. On the same principle, what Alexander regards as the indefinite apprehension of pure space-time as a whole is really the indefinite apprehension of the concrete universe as a whole including qualities and including mind as well as matter.

The Emergence of Qualities.

Let me begin by quoting Alexander's own formal and general account of what he means by "emergence" and by emergent qualities. "The world actually or historically develops from its first or elementary condition of Space-Time which possesses no quality except what we agreed to call the spatio-temporal quality of motion.[1] But as in the course of Time new complexity of motions comes into existence, a new quality emerges, that is, a new complex possesses as a matter of observed empirical fact a new or emergent quality."[2] To this we have to add that

[1] Which is not motion in any ordinary sense, but something which is *purely* spatio-temporal.

[2] *S. T. and D.*, vol. ii, p. 45.

qualities emerge in a certain serial order, so that the earlier precondition the occurrence of the later.

Now it is undeniable that a whole as such has characters different both from those of its own constituent parts and from those of the relatively separate factors which by their combination and interaction contribute to form it. But it is not *any* character which may thus belong to the parts and not to the whole. The character of the whole is limited and conditioned by the nature of the parts. This follows from the very meaning of the term *whole*. For a whole is nothing else than all its parts as such. It is simply identical with all its parts united and interrelated in the way appropriate to that kind of whole. The nature of the whole as such exists only in so far as the natures of the parts contribute to form it. It cannot therefore be disparate from that of the parts, as a colour is disparate from a smell. The integral character must be an integration of partial characters having the same generic nature: it must be a determinate of the same assignable determinable. Sounds united in a certain kind of complex whole form a melody; but they cannot form a smell or a colour. If a smell or a colour accompanies the process, it must be referred to other conditions than the mere union of the sounds. Now it seems to me that Alexander's conception of emergence as interpreted by his application of it involves this absurdity. It introduces as characters of a complex whole, due merely to its complexity, characters which are entirely disparate in their nature from the nature of the parts which in their union constitute the whole.

He starts from a purely spatial-temporal process[1] which increases in complexity as it advances. When it has reached a certain kind and degree of complexity, the complex as a whole acquires a new character. The character is new in the sense that it is no longer merely spatial-temporal.

If it is not new in this sense it has not the distinctive novelty of an "emergent quality", such as belongs to the so-called secondary qualities where they occur. On the other hand, if it does have this kind of novelty, it cannot be accounted for as a character of the complex. For in whatever way or degree a purely spatial-temporal process may become complicated, the complication is not itself a transition to anything which is not pure space-time. All the parts of the resulting complex still remain purely spatial-temporal whether considered severally or in the union with each other which constitutes the whole. If

[1] I have contended that such a process is an impossible abstraction. But I am here waiving this objection.

a new quality occurs which has not this generic nature it must be due to other conditions or to no conditions at all.

According to Alexander, the first qualities to "emerge" are those which constitute "materiality", *e.g.*, mass and energy. His position here may be interpreted in two alternative ways, and I am not sure which he means to adopt. He may mean that "materiality" is nothing but a complex development of pure space-time, so that it can be analysed without residue into purely spatial-temporal process. But if this is so, the purely spatial-temporal process has not the novelty of an emergent quality. On the other hand, if anything is supposed to occur which is not a purely spatial-temporal development, we are faced with the difficulty I have already stated. The novelty has no kind of relevance to the conditions from which it is said to emerge. It might as well be supposed to "emerge" from nothing at all.

The same difficulty presents itself in a more obvious and acute form when we consider the so-called "secondary" qualities of matter. The word "secondary" is misleading. The "secondary" qualities are the only qualities properly so called. Mass, energy, etc., are quantitative and relational characters. They are not qualities in the sense in which sweetness and redness are so, as we experience them in sense-perception. Now the difficulty of accounting for such qualities properly so called has been acute for all who have held in any form the mechanical view of nature which has been prevalent among men of science and philosophers from the time of Galileo and Descartes to the present. The essence of the mechanical view is the assumption that such non-qualitative factors as local motion, position, mass and energy are alone operative in determining the course of events in the physical world. It followed either that qualities have no physical existence or that they are somehow derived from or produced by such non-qualitative factors. The second alternative seemed to them, as it does to me, impossible. No *mere* combination, redistribution or transformation of non-qualitative factors can result in the existence of qualities. This was for them as it is for me an analytical proposition. Hence they were compelled to fall back on the first alternative. They accordingly deny that "secondary" qualities really belong to physical objects. They are perceived as qualifying the physical objects, but the perception is illusory. I have the idea of the paper before me as white; but it is really neither white nor any other colour. It has no quality at all in the proper sense of the word. All that really exists in the object is the power to produce such illusory ideas in us. Now this view is decidedly rejected both by

Alexander and myself. But we differ in our reasons. Alexander relies simply on the testimony of the senses. But as Descartes pointed out the senses are very fallible, and apart from other reasons we cannot safely assume that they do not deceive us even as regards the objective existence of qualities in general. On the other hand, Alexander entirely ignores and tacitly rejects the Berkeleyan argument which seems to me perfectly cogent. The Berkeleyan argument is that a world having only " primary qualities " (*i.e.*, quantitative and relational characters) is an impossible abstraction. Alexander on the contrary assumes that originally there was a universe empty of qualities. Hence he is bound to deal with the question : How do qualities arise in a non-qualitative world ? He attempts to answer it by his conception of emergence. But the question does not arise from experience, but from his own presuppositions. And these presuppositions are not only intrinsically absurd ; they are without a shred of empirical evidence. On the contrary, the concrete data of sense-perception show qualities everywhere and never mere extension, configuration and motion, and certainly never pure space-time. I accept " with natural piety " what is both suggested by the empirical evidence and required by the nature of the case. I assume that the qualitative aspect of the universe is as primary and universal as space-time, or as motion and configuration. New qualities occur as results of qualitative process, never of purely non-qualitative process. They no more do so than new motions and configurations result from purely qualitative process. Further, new qualities, like new motions and configurations, arise through *changes*, in which the old cease to exist in the transition to the new. Just so far as there is novelty, what previously existed vanishes. Previous positions of a moving body cease to be its positions as it passes to new ones. When red and yellow unite to form orange, the red and yellow as such cease to exist. The contrary holds good of what is called emergence. The emergent quality simply supervenes on and adds itself to the process from which it is said to emerge. The process might just as well exist and continue its course without the quality. If we suppose that the original space-time process was all that originally existed there is no reason why anything else should ever exist. If anything else does occur, it is a most misleading use of language to say that it emerges. All that we can say is that once it was not and now it is. But there is no process of change or transition through which it comes to be. In other words there is no process of emergence. But what is emergence if it is not a process ?

It can only be absolute creation—a concept which we should leave to the theologians.

Considered as a kind of causation, it is quite incapable of fulfilling the essential epistemological function of causation as what Hume calls "a principle of inference in matter of fact". If something occurs under certain circumstances, without any reason known or unknown why it should occur, there is no reason why it should occur again under similar circumstances in unobserved instances. As Hume pointed out, repetition of the conjunction, however frequent or uniform, makes no difference. In saying this I do not deny or belittle the logical value of repetition. My point is that inference to unobserved instances presupposes as a condition of its validity an inference which covers also the observed instances. Frequency and uniformity of repetition has logical value only inasmuch as it makes it probable or practically certain that A and B are causally connected and not merely conjoined—in other words, that there is a general *reason* why B should follow or accompany A, a reason common to observed and unobserved instances. But in what is called "emergence" there cannot be any such reason.

The view that qualities and qualitative process are as primary in the constitution of the physical universe as space and time and spatio-temporal process, is natural to commonsense, and if we except the atomists it was the prevailing view in Greek philosophy and science. What has stood in the way of its acceptance in modern times is a certain scientific prejudice. Galileo and his successors found that what alone lends itself to exact, systematic and comprehensive treatment by physical science is spatial and temporal process, with the quantitative and other relations which it involves. Tacitly assuming that nothing but what thus lends itself to the purposes of physical science can be physically real, they banished all qualities properly so called from the physical world. Alexander, less consistently, admits that they now exist in it, but denies that they belong to its original constitution. Hence his doctrine of "emergence". But the assumption on which both he and they proceed is baseless. There is no reason why the limitations of scientific method should coincide with the limits of real existence.

II.—PERCEPTUAL ASSURANCE (II.).

By D. R. Cousin.

Part II.

The Logic of Perceptual Assurance.

§ 9. *The problem.*

According to the view of perceptual consciousness which we have accepted from Mr. Price, the logical problem which it raises may be stated as follows. Our perceptual consciousness of material things is not intuitive knowledge nor is it reached by a demonstrative proof by deduction from causal or any other axioms. On the contrary, it is founded on a pseudo-intuitive taking-for-granted of the set of propositions elicited by the sensing of a sense-datum. But in spite of its origin it often justifies a high degree of rational assurance. How is this assurance justified ?

Stated with equal brevity, his solution to the problem is roughly as follows. Perceptual assurance is reached by the progressive synthesis [1] of a series of perceptual acts. When and in so far as what is presented to the mind throughout a series of such acts is the same material thing these acts support one another ; and because each such act involves the sensing of a sense-datum this mutual support of successive perceptual acts constitutes *confirmation* of the earlier by the later acts. The principle that the sensory element in each perceptual act constitutes *evidence for* the existence of a material thing is, according to Mr. Price, the logical foundation of the assurance which we reach by means of perceptual processes, and it is called by him the Principle of Confirmability.

The remarks which I wish to offer in this part of my paper are based on certain points which I tried to establish in Part I. I there tried to show, as a matter of the analysis of perceptual process, that it involves the entertaining of propositions about

[1] The phrase was suggested to me by Mr. Price as a possible alternative to ' further specification '.

sense-data, and that the confirmatory value of perceptual process rests on the verification of these propositions by the data sensed in the course of the process. Further, I suggested that we should distinguish the question what constitutes perceptual evidence for the existence of a material thing at all from the question what constitutes good evidence; and I suggested that the differences of reliability which we recognise between one perceptual process and another are connected with the different types of question which interrogative perceptual processes may be concerned to answer. Certain particular types of perceptual process, I tried to show, are relevant to particular types of question; and it is by a consideration of the type of question with which it is concerned and of the relevance to this question of the sensory element in the process, that we estimate the value of the process as a source of perceptual assurance.

I wish to begin (§ 10) by suggesting that the Principle of Confirmability is not really so mysterious as its title and the paradoxes with which Mr. Price introduces it suggest. If what I have said about the verification of propositions about sense-data is correct, sense-data (or rather propositions about sense-data) are evidence for material things (or rather for propositions about material things) in very much the same way as a proposition of any other sort is evidence for a hypothesis. I shall try to show this by comparing what I take to be the essence of the Principle of Confirmability with the account given by Mr. Ryle of the inductive evidence for a causal law. I shall next (§ 11) suggest a more general formula for the nature of evidence, a formula of which Mr. Ryle's formula and Mr. Price's Principle of Confirmability are alike special applications. In § 12 I shall try to show how this general principle can be applied to do the work of the Principle of Confirmability, and shall suggest a description which will show both the relation of the principle to the general principle of problematic inference and its special application to the logic of perception.

My next remark (§ 13) follows up what was said in § 7 about differences of reliability between one perceptual process and another, and results in the introduction of two minor principles.

Finally (§ 14), I wish to raise a question which has not so far been dealt with in this paper, and which is not discussed in the chapter of Mr. Price's book with which my paper is principally concerned. In his fourth chapter, Mr. Price discusses at length the theory that our consciousness of material things is reached by an inference from our sense-data as effects to material things as the causes of these effects. I agree with Mr. Price's rejection

of this theory, but I think that his criticism raises a question which it does not solve. For in the course of it he is led to elaborate four 'Methods' or types of argument which, though they cannot save the causal theory, yet appear startlingly life-like. Indeed as Mr. Price himself points out they are not unconnected with the description of actual perceptual processes, the sensory elements of which are elaborately examined in the eighth chapter of his book. The difference between the causal methods and the sensory series of Chapter VIII is that the former contain, as the latter considered in themselves do not, a consciousness of the material thing as the 'vertical' cause of the sense-data. Now it is one thing to say that the conception of material thing is no less original than the conception of vertical causality, and in saying this I am sure that Mr. Price is right. But it is another thing to say that the conception of vertical causality is wholly absent from the conception of a material thing which is present to our minds throughout a perceptual process; and I am not at all sure that, if Mr. Price holds this, he is right to do so. But if the conception of vertical causality is present in perceptual process it would be natural to inquire whether it affects the logic of the process, and if so in what way. This, then, is the topic of my concluding section.

§ 10. *Perceptual consciousness and causal generalisations.*

The Principle of Confirmability, to quote it once more, runs as follows: 'The existence of a particular visual or tactual sense-datum is *prima facie* evidence (1) for the existence of a material thing such that this sense-datum belongs to it, (2) for the possession by this thing of a front surface of a certain general sort' (*Perception*, p. 185). I should like to begin by suggesting some terminological changes which, while keeping—I believe—all that is essential to the principle, will make it easy to state some of the points which I wish to make. I should like to say that, in strictness, what we have evidence for is the *proposition* that there is such a material thing and the *proposition* that it has a front surface of such and such a general sort; and that the evidence for these propositions consists of certain other *propositions* about certain sense-data. The evidence for a thing, in this way of speaking, is not what makes it true, or makes it such as can reasonably be believed; it is something *the truth of which* makes that for which it is evidence true or probable. 'Being evidence for', then, is a relation between propositions, such that the truth of the one has a bearing on the truth of the other. The meaning

of 'having a bearing on' is what I am coming to in a moment; in the meantime what I am anxious about is the terms of the relation—they are propositions. The evidence for the propositions about material things about which we become assured as a result of a perceptual process is, then, the propositions about sense-data which are verified by the data sensed in the course of the process.

The importance of this terminological remark is as follows. It enables us to see more easily that the evidential value of the sense-data is not anything unique. They are important because the propositions involved in perceptual consciousness happen to be about sense-data, and consequently it is sense-data which verify them; if the propositions were about something else, as they are for instance in introspection, they would not have been verified by sense-data but by something else, perhaps by some mental act such as a judgment or a volition. Of course if they were not about sense-data or mental events or some other objects in relation to which there is direct knowledge they would not be directly verifiable, and their evidential relation to propositions about material things would be unimportant. Consequently it is a mercy that there are sense-data and these other things for us to entertain propositions about. But I cannot see that this question affects the logic of the inferences which we base upon our knowledge of them. This logical question, so far as I can see, is concerned wholly with the relation of the propositions which constitute the evidence to the propositions for which they are evidence. To this question the fact that they are propositions about sense-data is accidental.

What then are the relations constitutive of 'being evidence for'? Once we have stated the question in terms of a relation between propositions, certain analogies present themselves. What is the evidence for scientific generalisations? We are often told that it is their predictive value, *i.e.*, that the scientists deduce predictions from their hypotheses and if these predictions are verified regard the hypotheses as established more or less strongly. As Mr. Broad and Mr. Ryle have pointed out, the psychological surprisingness of this foreknowledge, and indeed the fact that the propositions deduced from the hypothesis refer to the future, though it may contribute to the convincingness of the inference and add to the prestige of the scientist, has nothing whatever to do with the rationality of the inference. An induction is just as well founded upon a comparable series of observations made in the past. The only thing that is of importance is the relation of the propositions verified to the hypothesis.

If this is correct, there is evidently a close analogy between the meaning of evidence in scientific generalisation and in perceptual consciousness. It will pay us, therefore, to examine an account of evidence, if we can find a good one, which has been designed to cover the case of inductive inference to causal laws. Fortunately a particularly clear and explicit answer to precisely this question is available. 'Any empirical proposition is evidence for a law', says Mr. Ryle, 'when, given the truth of the law plus that of some other empirical proposition, the truth of the first would logically follow; and any empirical proposition is evidence against a law when some proposition incompatible with it would follow in the same way. No further definition of "evidence for" and "evidence against" is required.' (G. Ryle, *Induction and Hypothesis*, in Proceedings of the Aristotelian Society, Supplementary Vol. xvi, p. 56.) In other words, anything which would be an instance of the law if there were such a law is evidence for there being such a law, and anything which would be incompatible with there being a particular law is evidence against there being such a law. This seems to me, as I gather it still does to Mr. Ryle, a good account of what evidence is in the particular case with which he is concerned. But before we can make further use of it in connexion with our own problem of perceptual assurance it requires to be generalised. In the next Section, with some help from Mr. J. M. Keynes, and with apologies to both Mr. Keynes and Mr. Ryle for any mistakes which I may be about to introduce into their views, I shall attempt this generalisation.

§ 11. *The Principle of Induction.*

A non-tautological proposition, I suppose, might be asserted without either direct knowledge of the fact which it purports to describe or any foundation in other propositions. Anyone who made such an assertion would be acting irrationally, and it is only in application to persons who act rationally that the question of evidence arises at all. Within the sphere of thinking, or the assertion of propositions only with some reason or ground, evidence seems to be both compared and contrasted with proof. If a rationally asserted proposition is not self-evident, nor based on direct knowledge, but is asserted with a reason which is to be found in other propositions, we may ask whether this reason amounts to demonstration, in which case the truth of the conclusion is as certain as that of the premisses, or whether the conclusion is only rendered probable by the premisses. If the

conclusion is only probable, we call the reason for it evidence rather than proof. In saying that it makes the conclusion probable we mean no more than that it is just not wholly irrational, given the truth of the evidence, to assert the conclusion. There is a difference between a conclusion asserted upon evidence and one asserted upon no grounds at all. But it is a far cry from evidence to proof; and there may be a great difference between evidence and good evidence. The class of propositions which are either proof of a given proposition or disproof of it or evidence for or against it, is the class of propositions *relevant* to that proposition.

Our next step is to formulate, under the guidance of Mr. Ryle's definition of the evidence for a causal law, a general account of the distinction between evidence and proof. This may, I think, be done in some such way as the following. A causal law is, after all, only one kind of hypothesis which we may seek to establish by inductive methods, though it is of course a very important one. But it does not differ as regards the logic of the process by which it is established from any other kind of hypothesis. The general formula of which we are in search, then, is a formula defining 'p is evidence for h' for *any* type of p and h.[1]

Now if we were looking for a definition of 'proof' our formula would be something like this:

$$\text{'}p\text{ is proof of }h\text{'} = \text{def. '}p\text{ entails }h\text{'}.$$

Since, however, we are in search of a definition of 'evidence' in a sense in which the position of a man who asserts a proposition upon evidence, while not so wholly irrational as that of a man who asserts groundlessly, is less strong than that of one who asserts on grounds which amount to proof, this is evidently not the formula which we want. Can we simply transpose the formula for proof and write

$$\text{'}p\text{ is evidence for }h\text{'} = \text{def. '}h\text{ entails }p\text{'}?$$

We cannot accept this without qualification. For propositions which are in need of support by evidence are rarely such as *by themselves* to entail any empirical propositions—any propositions, that is, which could form the basis of an inductive argument. (We may recall that the propositions about sense-data involved in perceptual consciousness were mainly concerned with *correlations* of sense-data with certain conditions. *Cf.* § 5.) It is

[1] I use h throughout for the hypothesis to be established, like Mr. Broad, and not, like Mr. Keynes, for the evidence in relation to which a conclusion is probable.

only in conjunction with some proposition asserting the fulfilment of certain conditions that the hypothesis entails the proposition which is evidence for it. The relations which constitute evidence, therefore, are expressed in the following formula:

'p is evidence for h' = $_{\text{def.}}$ '$(\exists q) : q \doteqdot p : q \,.\, h \,.$ ent $.\, p$'.

No doubt the necessity for this further empirical proposition is not as a rule obvious. The reason is that, as has so often been remarked before, interrogative perceptual processes take place against a background of normal perceptual process. Hence the conditions with which our sense-data are correlated may be taken as fulfilled; and what we are interested in is simply the data which, under these half-consciously recognised conditions, we obtain, and their relation to the hypothesis which we are for the time being considering. For the logic of this there is available an easy transformation of the formula suggested above. For, since

$$q \,.\, h \,.\supset.\, p : \supset : q \,.\supset.\, h \supset p$$

we can say

'p is evidence for h' = $_{\text{def.}}$ 'given q, h ent p'.

A similar formula would define 'evidence against'. This pair of formulæ, it seems to me, has some claim to be called the Principle of Induction. It has the advantage of representing problematic inference, as it should represent it, as being a species of formal fallacy, the fallacy of affirming the consequent. This is at first sight disturbing; but on reflection we see that it is unreasonable to expect anything else. If the inferences under consideration were not formally fallacious they would not be problematic inferences but deductive proofs.

§ 12. *The Principle of Perceptual Induction.*

If this is the general principle of which the Principle of Confirmability is a special application, what formulation should we adopt for the narrower principle? What we ought to require of the principle which states the foundations of the rational justification of perceptual assurance may be set out as follows. The principle should be a special application of the general notion of evidence; and it should lay it down that the evidence on which perceptual consciousness as distinct from other forms of cognition is based is the evidence of the senses. The second of these requirements is obviously satisfied by Mr. Price's Principle; for it unambiguously lays it down that the evidence for our perceptual assurance is the existence of such and such sense-data

observed in the course of the perceptual process under discussion. The first requirement, I suggest, will also be satisfied if we state the principle, in accordance with § 10 above, not directly in terms of sense-data but in terms of the propositions which these sense-data verify. Any proposition asserting the existence of a particular sense-datum, we may say, is evidence for the existence of a material thing such that, given the actual conditions of observation, the proposition asserting the existence of the material thing entails the first proposition. And any proposition asserting the existence of a particular sense-datum is evidence against the existence of a material thing if, given the actual conditions of observation, the proposition asserting the existence of the material thing would exclude the first proposition (where 'exclude' = 'entail the falsity of').

The argument of this and the two preceding sections has been intended to show that the principle on which perceptual assurance rests is not something unique, but that it is a specific form of a more general principle which is the foundation of problematic reasoning in general. If this is right, it would be as well to give it a name which indicates more clearly than the title given to it by Mr. Price its general logical character. It is tempting to give it some such name as the Principle of Perceptual Extrapolation. But this will not do. For to call it this would commit us to a particular theory about the nature of material things.[1] If this title is correct, it must be because the perceptual processes of which it is the principle are indeed processes of extrapolation. And this involves the view that the hypothesis which our evidence establishes is the existence of further entities of the same kind as those the existence of which constitutes our evidence. Since our evidence consists of sense-data, this would mean that material things consisted of sense-data. No process correctly describable as extrapolation could justify an inference from the existence of sense-data to the existence of anything possessing characters other than sense-given ones. That is to say, we should by adopting this title commit ourselves to Phenomenalism, and tacitly reject the possibility that material things may possess a constituent such as that which Mr. Price describes as a 'physical occupant'. Now we have resolved for the present to eschew the question whether or not Phenomenalism is true; and we cannot commit ourselves tacitly by the adoption of a title for our principle which presupposes the phenomenalist view. I suggest, therefore, that we should call our principle the *Principle of*

[1] I owe this correction to Mr. Price.

Perceptual Induction. Some such principle, I agree with Mr. Price, is absolutely necessary if our process of perceptual confirmation is to get going at all.

It must be admitted, I think, that our principle would make the propositions about sense-data which we verify in the course of a perceptual process evidence not only for the existence of material things but equally for any other hypothesis which would entail the same propositions about sense-data. If the thoughts of God, or the activities of a society of Monads, entailed just those propositions about sense-data, then the verification of the propositions about sense-data would make it so far reasonable to believe in certain propositions about the thoughts of God or the activities of a society of Monads. But I do not think that this is very important. We do not need to know whether the world represents the thoughts of God, the activities of Monads or (what some of us have suspected all along) nothing but itself. What we need to know, and we need this very badly, is *what in particular* God is thinking and about to think, *what in particular* the Monads are doing and about to do. And since this, it appears, is quite the same whether it is thought by God or performed by Monads, the metaphysical question, for the present at least, can wait.

§ 13. *Comparative reliabilities.*

The Principle of Perceptual Induction, if what has been said above is right, is the logical foundation of perceptual assurance. But it is not clear that the last word has been said on the logic of the matter when this principle has been established. As Mr. Broad has shown, we may know that our evidence favours one hypothesis rather than another, or that the evidence in favour of one hypothesis is stronger than that in favour of another, and yet be unable to see that the evidence is sufficient to lend any *high* probability to the conclusion. Now I do not think that we can see, without some further explanation, how our principle can enable us to distinguish as we actually do distinguish between one perceptual process and another in respect of reliability, far less how our principle justifies us in holding that the probability of conclusions based on the evidence of the senses is ever high. I am not at all sure of the answer to the last question, though I shall say something which I believe is *relevant* to it in the next section. It seems possible, however, that some light may be thrown by a study of the easier question about the comparative reliabilities of different particular perceptual processes, and I propose to say something about this in the present section.

The ground for what I am about to say has been to some extent prepared in § 7. There are differences of reliability between perceptual processes. Some result in a well-founded assurance, while others either leave us guessing or without disturbing our animal faith leave us in a state which could be disturbed by the first challenge. That there are these differences is to some extent a brute fact, for they are conditioned by the mere matter of fact that the peripheral data belonging to different material things may be very much alike while their nuclear data are commonly very different. But it is a fact which has logical implications. For the difference between these processes is by no means merely a difference in psychological confidence ; it is a difference in the rationality of the inference in the several cases. And such a difference presupposes a difference in the character of the evidence in the several cases. What is the logical nature of this difference in evidence ?

It was suggested in § 7 that the answer must be on some such lines as the following. Leaving aside the obvious cases where of two processes involving data of the same degree of internal and external diversification one is better than the other because founded on the sensory verification of a larger number of propositions, we base our judgments of reliability on the relevance of the propositions sensorily verified to the particular type of question characteristic of the processes respectively. Where the question to be settled concerns the particular kind or individual identity of the material thing evidenced, the propositions most directly relevant will be those concerned with the determinate character of the surface sensibly manifested, and these will be most definitely verified by highly variegated data. Where the question, on the other hand, concerns the real or illusory character of the thing evidenced, the sensory evidence most required is that of the propositions defining a complete set of spatially constructible surfaces, and in other ways making up the complete definition of a material thing ; the determinateness of the thing evidenced is less important.

We might express the gist of this in the form of two subsidiary principles, the Principle of Perceptual Discrimination and the Principle of Perceptual Authentication. These might run as follows. *Principle of Perceptual Discrimination :* Of two perceptual processes that one gives the better assurance of the particular nature or identity of the material thing evidenced which involves the sensory verification of propositions consistent with fewer alternative hypotheses about the nature or identity of the thing in question. *Principle of Perceptual Authen-*

tication: Of two perceptual processes that one gives the better assurance of the real existence of some material thing, which involves the sensory verification of propositions defining a more complete set of mutually consistent supplementary characteristics of the thing in question.

It is to be observed, however, that the bearing of these different types of perceptual process is not exclusively upon the types of question to which they are more particularly appropriate. For there is, I suppose, little doubt that a process which was good by the standard of the Principle of Perceptual Discrimination would in general be better for the purposes of authentication than a process which was less good by the same standards. And it is even possible that a process which was relatively poor in supplementation might, if very good from the point of view of the determinateness of the hypothesis evidenced, be better than a process relatively richer in supplementation, though it would be impossible to measure this superiority. For a process which is evidence for a highly determinate hypothesis about a material thing is *a fortiori* evidence for a less determinate hypothesis: if there is evidence for the view that this is my cat, *a fortiori* there is evidence for the view that it is some sort of cat, or at any rate some body.[1] And conversely, as Mr. Price points out, evidence which is pretty complete in respect of supplementation may nevertheless be misleading; a mirage might maintain itself as long as we rode round it at a distance of three miles or more but vanish on a closer approach (*Perception*, p. 197). I do not see at present that we can lay down any logical principle to this effect. We might say that for purposes of authentication processes based on nuclear were better than processes based on peripheral data. But I do not see how this could be more than an inductive generalisation, based on the observation that things seen from a distance have more often turned out to be illusory or hallucinatory than things seen from near at hand.

There is indeed one condition under which I can see a way to make this a logical principle. This is by assuming vertical causation. If it is the case that part of the hypothesis which we are entertaining when we take for granted that there is a material thing is the proposition that there exists some physical occupant which is the differential condition of the sense-data which belong to the thing, and if, moreover, this physical occupant is spatially coincident with the standard solid of the thing, then, but not so far as I can see otherwise, it will follow that nuclear data are a better foundation than peripheral ones for perceptual

[1] On this point *cf. Perception*, p. 194.

assurance of existence. (On this point *cf. Perception*, pp. 80-89, on the Methods of Sources and of Indispensables.)

So far as we have yet gone, however, this assumption appears to be inconsistent with Phenomenalism; and we must leave the question an open one. More will be said of it in the following section.

Before leaving the Principles of Perceptual Discrimination and Perceptual Authentication, however, and any corresponding principles which may be laid down for the guidance of other types of perceptual process, there is a general observation which it seems worth while to make. Leaving aside the comparatively uninteresting cases of perceptual processes which owe their differences of reliability to mere quantitative differences, *i.e.* to their respectively greater and less duration, the differences which we have considered have all been concerned with differences in the hypothesis evidenced. Either the hypothesis evidenced in a better process is more determinate or it is more complete than that evidenced in a worse one. Now what, we may ask, is meant by the evidencing of a more determinate hypothesis? What is meant is that our process constituted evidence for some one hypothesis, instead of for *any* of a number of alternative hypotheses. And it can do this only by verifying propositions about sense-data *inconsistent* with some of these alternative hypotheses, in other words, by *eliminating* these hypotheses. Now this of course is just what we should expect if the eliminative view of induction is correct. And it would not be difficult to bring the other cases under the same general account. For we can regard the possibility of illusion and that of hallucination as hypotheses co-ordinate with that of the real existence of a material thing; they will entail propositions about sense-data which differ in more or less precisely assignable ways from those entailed by the hypothesis of real existence. And the verification of the propositions entailed by real existence can be regarded as falsification of the propositions entailed by some varieties of the hypothesis of illusion or hallucination. Now I do not wish here to pronounce upon the question whether or not induction is always and necessarily eliminative. But it seems to me clear that its eliminative aspect is present in much of the best perceptual process, and that it has a great deal to do with the logic of its reliability. The more alive we are to the possible alternatives, and the more of these alternatives we are able sensorily to refute, the stronger our position is. For, while no amount of evidence, perhaps, is ever sufficient to establish conclusively the existence of a particular material thing, it is clear that a finite amount of

evidence may satisfactorily *refute* the hypothesis of its existence. So that a perceptual process which is consciously eliminative, while it cannnot constitute knowledge of its conclusion, may *contain* a fair amount of knowledge which is relevant to that conclusion. This is one more illustration of the importance, for our understanding of perceptual consciousness, of the realisation that each process of developed consciousness takes place in conscious comparison and contrast with the other processes which together make up the texture of perceptual experience.

§ 14. *Vertical causality.*

In formulating the Principles so far concerned, we have been careful not to commit ourselves to Phenomenalism, but on the contrary to leave room for the hypothesis that material things include, as well as a family of sense-data, some *other* constituent which might be described as a ' physical occupant '. The main business of this physical occupant is to be the subject of causal characteristics. Now the most obvious causal characteristics are manifested, as Mr. Price has rightly maintained, in horizontal causation, *i.e.* in the effects which one material thing produces upon other material things, whatever may be the correct analysis of this way of speaking. But of course it is also possible to hold, and it has been held by many philosophers and perhaps by most scientists, that the causal characteristics of the physical occupant are manifested also in relation to the sense-data belonging to the material thing of which it is a constituent. This causality, called by Mr. Price ' vertical causality ', has, of course, been made the basis of a theory of perceptual consciousness which he has strongly criticised. It is not my purpose in this paper to offer any general discussion of this alternative to Phenomenalism ; but there is a particular question arising out of the existence of this alternative hypothesis which is directly relevant to the subject of this paper. It is this. Would the assumption of vertical causality affect the logic of perceptual assurance, and if so in what way ? [1] This question can, I think, be discussed without any direct pronouncement on the more general issue ; and, if so, my treatment of the logic of perceptual assurance would obviously be incomplete without some discussion of this topic. I am inclined to think, moreover, that there is a further reason why the topic should be discussed. For while I agree that we do not reach the conception of material things by starting from

[1] I am particularly indebted to Mr. Broad's articles in MIND, 1918 and 1920, in connexion with the argument of the present section.

a conception of causation which is independent of it, it appears to me that the notion of vertical causality is a *part* of what we take for granted when we are perceptually conscious of a material thing. We do not, certainly, distinguish clearly between the sense-datum as effect and the physical occupant as cause; but we do, I think, feel in a vague way that the thing we see *makes* us see it, and makes us see it thus and not otherwise. For these two reasons, then, I propose to discuss the question what effect the conception of vertical causality has upon the logic of perceptual assurance.

The question which I am proposing to myself is not, at least in form, the question 'What reason have we to suppose that the sense-data belonging to material things are always caused by the physical occupant associated with the thing?' It is the question 'What, in particular cases, is contributed to the logic of my assurance that this is my pen and not yours, far less an elephant, or that it really is my pen and I am not suffering from an hallucination, by the notion that there is some vertical cause of my sense-data?' It seems to me very probable that the answer to the second question is the only answer of which the first is capable; but the decision on this point lies beyond the scope of this paper. (On this point compare Mr. Broad's articles on Induction and Probability in MIND, 1918 and 1920.)

I will begin by indicating a feature of the logic of perceptual assurance as so far outlined here which seems to leave some scope for the beneficent intervention of the conception of vertical causality. We have seen already, in considering the comparative reliabilities of different perceptual processes, that the hypotheses which processes of different kinds profess to establish exhibit different degrees of determinateness. It can hardly have escaped notice at the same time that all of these hypotheses are extremely *complicated*. Let us consider for the present only what is entailed in the way of propositions which are in principle capable of direct verification, *i.e.* of propositions about sense-data. It will be sufficient illustration to consider only the propositions concerned with shape. The proposition 'This is my pen' entails one set of propositions defining a perspective series obtained by looking at the top of the cap from different distances, another defining a perspective series obtained by looking at the whole length of the pen from different distances, and others defining an indefinite number of perspective series each obtained by varying the distance of the pen while maintaining constant some of the other infinite possible obliquities. It entails, moreover, a set of propositions defining a Progressive Adjunction Series at a distance

of six feet, another defining a progressive adjunction series at a distance of six inches, and indefinitely many others. It entails one set of such propositions for me, and another for an observer with a different index of phenomenal regression. And of course it entails similar sets of propositions taking care of data belonging to the other senses, touch, sound, smell, etc., and for the different sensory equipment of all possible observers, animal, human, angelic and divine. Moreover, we must allow for the propositions which are not directly verifiable, *viz.* those which assert the possession by the thing of characteristics other than sensible ones. All this has to be entailed by the harmless-sounding proposition 'This is my pen'. And, for all that we have said to the contrary, *every one of these propositions is logically independent of any of the others.* They are collectively entailed by 'This is my pen'; and collectively they entail some particular set of propositions which has just been verified by my sense-data. But for all we have said to the contrary there is not so much as a material implication, far less an entailment, between any of them and any of the others singly or conjunctively. There is, so far as we can see, no *reason* why the data correlated with different distances from the same surface of the pen should show the sorts of regular variation in size and shape that they do show. Perspective series might perfectly well show the kinds of difference between adjoining members that we notice when we turn our heads rapidly, or even greater differences, and so on with all the other kinds of series. If things were like this, life would be very difficult; it would be very hard to remember what any particular kind of thing was like, even supposing that the concept of material thing did not simply rest unused in some pigeon-hole of our minds. We are not concerned at present, however, with the inconveniences which would follow if things were arranged as, fortunately, they are not. We are concerned only to note that the hypothesis that this is my pen, though it often seems to be established, is in its present form an extremely complicated one.

The bearing of this on the logic of perceptual assurance is as follows. We have shown that the perceptual processes which we actually experience are comparable among themselves in respect of greater or less reliability, in proportion as the hypotheses verified are of greater or less determinateness. But the considerations advanced in the last paragraph show that even so the antecedent improbability of the hypothesis verified by any perceptual process is enormous. It remains very difficult to see, therefore, how any finite perceptual process can establish the existence and nature of a material thing with any appreciable reliability.

The point may perhaps be reinforced by pointing out that perceptual process as so far described is a process of *extrapolation*, and a somewhat extravagant one at that. Extrapolation is at best a hazardous process, very much more hazardous for instance than interpolation. And what little validity it has belongs to it only so far as it is held to justify the attribution to events beyond the range of observation of characters *similar* to those of the events actually observed. But perceptual processes, if what has just been said is correct, do not conform to this canon. For they are held to be evidence for the existence of a *family* of sense-data, and confamiliarity is consistent with a high degree of *dissimilarity* among the confamiliar data. As Mr. Price has pointed out, data belonging to different families may have far more resemblance to one another, *e.g.* in the case of front views of houses in different towns, than data belonging to the same family, *e.g.* in the case of the front and the side view of the same house. Needless to say, any argument from propositions about sense-data to *non-sensible* characteristics of material things would be proportionately weaker.

What is the bearing of all this on the question of vertical causality ? It is this. So long as we think of the various propositions entailed by the existence of a particular material thing as logically independent of one another, the hypothesis to be established by any perceptual process is very complicated, and its antecedent improbability is therefore very high. This points to a gap in our theory as at present formulated. This gap would be filled by any conception which had the effect of simplifying our hypothesis, and in this way increasing its antecedent probability. Can the assumption of vertical causality do this ? I think that it can.

What, in outline, does the assumption of vertical causality amount to ? The essentials of the conception, I take it, are as set out in Mr. Price's chapter on the Causal Theory of Perception (*Perception*, ch. iv), with perhaps a few modifications in the light of his chapters on the relations of sense-data to one another and to matter. We are to think of any given sense-datum as produced by the interaction of a number of different cause-factors. In the case of a visual datum, the chief among these are the constitution of the percipient, the diaphanous medium, the source of light and the physical occupant connected with the material thing to which the datum belongs. We are to think of these cause-factors as capable of varying independently, and as varying sometimes separately and sometimes together. The percipient, the medium and the source of light remaining the same, altera-

tion of the physical occupant is to produce alteration of the datum. And conversely the percipient, the source of light and the physical occupant remaining the same, alteration in the medium (*e.g.* alteration in the quantitative characteristics of the medium as we approach or recede from the object) is to produce a change in the datum. In order to state the rest of the position clearly, we want a distinction between standing and differential conditions of the existence of sense-data. Mr. Price's definition of these terms is formulated from the point of view of a single observer perceptually conscious of a multiplicity of different material things, and it is therefore a trifle awkward in the present context where we are discussing the causation of all the sense-data belonging to a single thing. But as we shall have to adopt Mr. Price's point of view in a moment I propose to adhere to his formulation of the distinction. We shall say, accordingly, that the physical occupant in question is the differential condition of all the sense-data belonging to a certain material thing, and that media, sources of light, observers, etc., are standing conditions. The essence of the hypothesis of vertical causation is this assumption that all the sense-data belonging to a single material thing are produced by the interaction of different values of standing conditions of these various sorts with a single differential condition.

What is the effect of this assumption upon the hypotheses to be established in perceptual consciousness? It is, I think, as follows. The assumption allows us to reject the view that the propositions entailed by any proposition about a material thing are independent of one another. Instead, we are to think of them as being for the most part *deducible* from some finite number of propositions about the physical occupant which is the differential condition of all the sense-data belonging to the material thing, together with certain law-propositions correlating the characteristics of the data belonging to the family in question with the various possible values of the standing conditions upon which, together with the differential condition, they depend. The mere assumption of vertical causality, of course, does not furnish us with any clue to the actual laws involved, any more than it informs us of the characteristics of the physical occupant associated with any particular material thing. It amounts to no more than the assumption that such a deduction of the characters of the data belonging to the family which is one constituent of the thing, is in principle possible. It amounts, that is, to saying that the propositions entailed by the perceptual consciousness of any particular material thing are not, as they appeared to be, independent. Instead, they form part of what is, in relation to

some finite but as yet unknown number of basic propositions, the deductive system constituting our knowledge of the physical world. And this, it may be held, increases the antecedent probability of any given hypothesis about the existence and nature of a particular material thing.

These general considerations will become clearer if we now examine some particular perceptual process, with a view to discovering the part played in it by the conception of vertical causality. Let us suppose that our perceptual process has been based on a perspective series of data, *i.e.* that it has involved the verification of progressively more determinate propositions belonging to the sub-set concerned with one particular surface of the thing. Now logically our inference contains two parts: (1) the inference that the series of data sensed are due to a common differential condition; (2) the inference that since this differential condition exists other data of particular sorts are obtainable. Now part (2) is simply the meaning of the hypothesis of vertical causality; the precarious part of the inference, therefore, is narrowed down to part (1), the inference that the data sensed are due to one and the same differential condition. It is to this part of the argument, then, that we must direct our attention.

We are not to discuss the question whether these data are or are not due to some set of conditions, including a physical occupant of some sort. That is the general hypothesis the effect of which we are to examine. What we have to do just now is to set out the effect of this assumption in determining the form of some particular argument. The answer appears to be, in brief, that we suppose these data to be due to the same differential condition because we have varied such of the other conditions as seemed to be in our power, and have observed variations in the data which might reasonably be supposed to be correlated with these variations in the standing conditions, but nevertheless there has remained a certain residuum of similarity in the data in question (the similarity which Mr. Price describes by saying that they are variations upon a common theme), and of similarity in respect of characteristics in respect of which they differ definitely from other data in the set of sense-fields to which they belong. It is this residuum of what we may call specific similarity which justifies us in referring these data to a differential condition which is the same for all of them and different from the corresponding condition of other material things of which we are conscious in the same perceptual acts.

Now this inference may certainly be mistaken. The data may not all be due to the same differential condition; they

may be due, like the shadows on the screen at the cinema, to a succession of different physical occupants. But it has, compared with the extrapolative inference from the same set of data to an enormous number of independent propositions, all the excess antecedent probability of the more modest interpolative argument. It is true that if we were considering the argument from this set of data to a physical cause-factor, *i.e.* to a cause-factor having characteristics coming under some quite different determinable from that exemplified in the characters of the sense-data themselves, our argument would be, so to speak, *qualitatively* extrapolative, *i.e.* it would not be even extrapolative but something much weaker. But that is not the argument that we have to consider. Our argument is the argument that, given that each datum is the resultant of the interaction of a number of different cause-factors, the observed specific similarity of the data is evidence for the assumption that one of these cause-factors is common to all the data of the series. And that is an interpolative argument.

I conclude then that the assumption of vertical causality, which as I have indicated I believe to be a part of what we all take for granted when we are perceptually conscious of a material thing, has an effect on the logic of perceptual assurance. This effect is the reduction of the antecedent improbability of the hypothesis which any perceptual process is required to establish. It reduces the antecedent improbability by replacing an excessively complicated by a still complicated but relatively far simpler hypothesis: it gives us, that is to say, instead of an indefinitely large number of independent propositions collectively entailed by the thing-proposition to be established, some finite number of propositions about the characteristics of the physical part of the material thing in question, from which, together with some finite number of laws, the whole set of propositions defining the family is in principle deducible.

All this, I think, both ought to be and actually is tautological. A cause is just whatever makes it possible to deduce what without it you would have to accept as brute fact, or perhaps to remain in ignorance of. So that in saying that the hypothesis of vertical causality converts the set of independent propositions which defines a family into a partially deductive system we are merely explaining the meaning of vertical causality. If this is true, we can see another reason for including this discussion of vertical causality in our present study. For vertical causality, if this is right, is not a hypothesis *alternative* to phenomenalism; it is, on the contrary, a purely logical assumption of which Phenomenal-

ists will have to give their own account. The only thing that is irrelevant to our inquiry is the question how the phenomenalist account of vertical causality will differ from a materialist or agnostic account.

§ 15. *Summary of Part II.*

After stating the problem about the logic of perceptual process, and outlining the general nature of the remarks I proposed to make upon Mr. Price's solution (§ 9), I suggested a reformulation of the Principle of Confirmability in terms of the propositions verified by the sense-data involved in perceptual process. I tried to show, with the help of this reformulation, that the problem is not unique, but is on the contrary only a special case of the problem of inductive inference, co-ordinate with the problem of the inductive establishment of causal laws, a principle for which has been recently formulated by Mr. Ryle (§ 10). The next section (§ 11) was devoted to an attempt to generalise Mr. Ryle's formula, and § 12 to formulating the principle applicable to the special case of perceptual induction. The question of the comparative reliabilities of different perceptual processes next engaged us, and we saw that differences in the determinateness and in the completeness of the propositions verified determined differences of reliability, in a way quite in accordance with the eliminative theory of induction (§ 13). Finally (§ 14), the question of vertical causality seemed worth raising, for the sake of its bearing on the antecedent probability of the hypothesis of material things in general. It was suggested that the conception is not absent from our ordinary perceptual consciousness, and that it could contribute something to the probabilification of perceptual consciousness. For that reason it has to be reckoned with in any analysis of material-thing-hood.

III.—KNOWLEDGE, REALITY, AND OBJECTIVITY (I.).

By H. F. Hallett.

The purpose of this discussion is to lay bare some fundamental difficulties in the common conception of "knowledge" as a subject-object relation; to consider their implications for our account of the real; and to indicate what I take to be the metaphysical etiology of "experience" and its objects. I make no secret from the start, then, that I believe the subject-object experience, that idealists have taken to be ultimate, to be derivative, and the "subject" to be ontologically prior to the object. I am not so naïve, however, as to suppose that the subject *qua* knowing objects is epistemologically prior to the objects that it knows: if we limit ourselves to experience we thereby make subject and object co-ordinate; but to argue from this derived co-ordination that experience is a limiting conception is a plain and vicious circle. To take subject and object in experience as *ontologically* co-ordinate is, in my view, the fallacy of idealism; just as it is the fallacy of realism to take the object for the real. The "subject" that I claim to be ontologically prior to the object is primarily the "self" that among its constituent actions includes that of "knowing" its "other"; but also, and equally prior to the object, there is the "other" that among its constituent actions includes that of "manifesting itself" to the "self". The "manifestation" of the "other" answers to the "knowledge" of the "self"; and "self" and "other" have concourse, partly in the direct possession of the "other" by the "self" in "enjoyed" "community" (which is knowledge *par excellence*), and partly in the objective appearance of the "other" to the "self" which "contemplates" it. This is knowledge commonly so-called, *i.e.*, a relation of subject and object, but is only excusably so-called in so far as in its perfection it involves the complete apprehension of the object, though not of the real as a thing-in-itself. I shall propose that this objective appearance or "object" is a joint expression of the "self" and its "other", owing factors to each, factors which if isolated would not be objects at all, but which become such in their union.

I.

Knowledge and Reality.

Let me begin with something relatively rough and commonplace: to believe a proposition, or to believe a fact, means to accept the proposition as true, or the fact as genuine irresistible datum. But the actual truth of the proposition, or the genuineness of the datum, does not give us the right to call our belief "knowledge", for we may, and perhaps often do, accidentally believe what happens to be true though our belief in it was ill-founded.[1] Our belief in what is true is only rightly called "knowledge" if the belief is well-grounded or certified. The term "grounded" suggests a demand for rational grounds or premisses, *i.e.*, for demonstration, and it is clear that in principle this cannot always be provided, or even rightly desiderated. All demonstration leads back to indemonstrable bases, and grounds must themselves be grounded on what is self-grounded. We are thus led back to propositions and data of such a nature as to be self-evident or indubitable. Thus though there may be conditions under which true belief is knowledge, yet belief in what is true, *i.e.*, the acceptance of what is actually the case as being so, or the taking as fact of what is fact, is not in itself knowledge except where the truth is self-evident or the fact ineluctable. In all other cases grounds must be provided leading up to such self-evident propositions or indubitable facts. Thus all knowledge depends in the end upon our acceptance of a form of knowledge that is indubitable. My immediate purpose is to inquire whether such a form of knowledge is to be found, *i.e.*, whether and, if so, how, it is possible, and whether and, if so, in what form, it is discoverable in human experience.

I must, however, make one important limitation if I am to keep the discussion within the limits of my space: the distinction between our knowledge of truths and principles and our knowledge of matters of fact is long-established, and evidently has a *prima facie* validity even for those who do not accept its ultimacy; and I propose to restrict my inquiry, so far as possible, to our reputed knowledge of matters of fact where the relation of knowledge to reality is most direct and crucial, not to say familiar, and the notions of reality, being, and existence can be taken as congruent. There is also a reason other than that of

[1] *Cf.* Descartes, *Medit. IV*: "If I affirm what is not true, it is evident that I deceive myself; even though I judge according to truth, this occurs only by chance". *Cf.* also Russell, *Problems of Philosophy*, pp. 205-207.

space for setting this limit to our inquiry, *viz.*, that it will formally exculpate us from the charge that has often been levelled against those who attempt to estimate the validity of knowledge *ab extra*: that they are employing cognitive principles, well or ill, and thus implying their trustworthiness, in their criticism of knowledge; that they are thus assuming the validity of knowledge in the very act of questioning it; and that therefore if their result is negative it dubitates itself, while if it is affirmative it is so far analytical and thus inconclusive.[1] By confining ourselves to knowledge of matters of fact we may escape between the horns of this epistemological dilemma, for we may legitimately continue to suppose that our use of cognitive principles, whether regulative or even constitutive, raises no special difficulties relevant to our immediate inquiry, and may thus concentrate our attention more strictly upon its crucial point, *viz.*, the relation of knowledge to real existence in that region of our reputed knowledge of matters of fact that has been, or may be, held to be immediate and indubitable. The precise point at which this guarantee applies need not yet be determined.

Everyone is familiar to-day with some analogue or other of the distinction between that part of our knowledge which is epistemologically immediate and that remainder which is mediate. The distinction in its modern form goes back, I think, to John Grote [2] who used the terms "knowledge of acquaintance" and "knowledge about"; though similar distinctions are signalized in several ancient and modern languages by the use of different terms for analogous distinctions of apprehension.[3] Grote's doctrine was seized upon and emphasized by thinkers as widely

[1] *Cf.* B.D.S., *De Intell. Emend.*, §§ 30-32; *Eth.* II., xliii, Sch. As this is an objection that I have myself often raised perhaps I ought to say that I now think that it is valid only as applied to the imagined doubts of a supposed perfect knower. Its weakness lies in the supposition that such a knower could have doubts, and also, more important as we shall see, in the radical defect of subject-object knowledge through which a perfect knower would necessarily be condemned to perfect ignorance. In Spinozistic terms: the one thing a "perfect being" cannot be is an "object"; thus the object of a perfect knower would be no object at all. Of course, though the *terms* are Spinozistic, the *doctrine* is not: Spinoza, I think, usually conceives his agents as objects that are active; and his objects as real in proportion to their agency: and this is true even of his "perfect being".

[2] *Exploratio Philosophica*, p. 60.

[3] *E.g.*, *connaître, savoir; Kennen, Wissen; noscere, scire*, etc. In view of the vagaries of linguistic usage the question of the exact equivalence of these distinctions with each other and with that of Grote need not detain us.

divergent as William James [1] and Bertrand Russell,[2] and it is without doubt of great importance, even if it is not as exhaustive as has sometimes been supposed. Russell expresses the distinction as lying between "knowledge by acquaintance" and "knowledge by description", of which the former is epistemologically superior. Indeed he is ready to define knowledge wholly in terms of acquaintance, going so far as to assert that the object to which a description applies "is not, strictly speaking, known to us at all".[3] Knowledge by description is knowledge *of* a description and *that* the description applies to an object. Acquaintance with objects, on the other hand, "essentially consists in a relation between the mind and something other than the mind; it is this that constitutes the mind's power of knowing things".[4] Knowledge by description, however, is much more ample than our knowledge by acquaintance, for it can enable us to apprehend the existence of things with which neither we nor anyone else are acquainted. But it rests upon acquaintance, as well as upon our "knowledge of truths", which again are grasped by "intuition" or derived inferentially from such intuitions: "intuition" being the form of acquaintance appropriate to principles and truths. "The fundamental principle in the analysis of propositions containing descriptions is this: *Every proposition which we can understand must be composed wholly of constituents with which we are acquainted.*" [5]

Let me point out at this early stage, and in no polemical spirit, the special difficulty in which the knower is placed according to this account of his relation to reality: knowledge in the full sense means direct acquaintance with the object; but this, to speak with sobriety, is limited in scope and depth. "I know the color blue when I see it, and the flavor of a pear when I taste it; I know an inch when I move my finger through it; a second of time, when I feel it pass; an effort of attention when I make it; a difference between two things when I notice it; but *about* the inner nature of these facts, or what makes them what they are, I can say nothing at all. I cannot impart acquaintance with them to any one who has not already made it himself. I cannot *describe* them, make a blind man guess what blue is like, define to a child a syllogism, or tell a philosopher in just what respect distance is just what it is, and differs from other forms of relation." [6] My acquaintance gives me the "that" and the incommunicable superficial "what", but not the real inner nature of the object,

[1] *Principles of Psychology*, ch. viii. [2] *Problems of Philosophy*, ch. v.
[3] *Ibid.*, p. 75. [4] *Ibid.*, pp. 66-67. [5] *Ibid.*, p. 91.
[6] James, *Principles of Psychology*, i, p. 221.

not what we want always to know *about* it: what makes it what it is. The more we pursue the inner nature of the object by description, the further are we removed from certain knowledge of it; the more we seek an indubitable apprehension of it, the more we are compelled to rest content with what is known by direct acquaintance. Acquaintance is knowledge-*par-excellence* of superficial fact; description is dubious knowledge of fact-*par-excellence*.

For the present, however, we may let this pass as no more than another expression of the characteristic limitations of human knowledge. At least in our knowledge by acquaintance we are said to have genuine knowledge in the full sense, though of a range of objects limited in scope and, it will be admitted, relatively superficial. If this can be established it will constitute no mean barrier to epistemological scepticism. But can it?

There is an argument that has often been used by neo-Kantian idealists and by those who, like myself, have come under their influence, to the effect that no genuine cognition can be purely immediate; that the purest piece of knowledge by acquaintance is already infected with mediacy. It is a "such and such" and no other; it is a "seen", or "heard", or "felt", momentary or enduring, simultaneous with or successive to another; it is "now" and not "then", extended or restricted; and so on. And if these elements of mediacy are rigorously excluded, and we restrict ourselves to the absolutely pure immediate datum, there remains nothing at all: a "whatless that". I do not, however, wish to make use of this argument; for though I agree that it has point in the analysis of knowledge-as-such, emphasizing the essential co-operation of "self" and "other" in the generation of the "object" that is related to the "self" as known and to the "other" as appearance, yet it can hardly be regarded as *ad rem* in our present discussion that is directed to *human* knowledge.[1] For it is plain that we are directly acquainted with

[1] To take the formal analysis of the genesis of the objects of knowledge-as-such as an account of factors that explicitly co-operate throughout the whole range of empirical objects would be to overlook the special characteristics of human knowledge entailed by man's place in the hierarchy of being. It is a mistake to suppose that wherever there is a knower his knowledge explicitly exhibits the marks of its metaphysical genesis, and is limited only in scope, and not in character. I have sometimes been asked why I make so much of the distinction of human knowledge and knowledge-as-such, when knowledge must always be of the same character, varying only in the range of its objects: I reply that one of the marks of the finiteness of human knowledge is the concealment of much of its mediacy within the ostensible immediacy of its elementary objects. Knowledge varies not only in its extent but also, and even more important, in its profundity, in its insight into the essence of the real "other" achieved

certain objects as distinct from the wider and more dubious range of objects that we suppose ourselves to know something about, though it may be true that what is psychologically immediate includes factors of epistemological mediacy.[1] The ques-

through its *complete* apprehension of the "appearance", generated by the community of "self" and "other", that we call "object". The immediate objects of human cognition may thus be both objects and immediate in spite of the formal analysis of objectivity. The development of cognition that is constantly aimed at in knowledge by description desubstantializes the objects of immediacy by the further and ostensible development of their implicit mediacy. We thus conceive two limits of development: on the one hand, that of a "knower" for whom the "datum" would be a "thatless what", constituted by mediacy that mediates nothing—a pure possibility, *objectively* indistinguishable from nonentity; and on the other hand, that of a just-awakening consciousness for which all mediacy is implicit in the "one confused Fact" which is datum (James, *Principles of Psychology*, ii, p. 8). Below this, in a complete formal survey, there would be that of a "knower" for whom the "datum" would be a "whatless that", constituted by immediacy, devoid of all mediation, explicit or implicit, and thus complete nonentity. Knowledge as contemplation of objects thus disappears at both the upper and lower ideal limits; it belongs to the middle stages of epistemological development, that is, to the consciousness of the finite individual. And it cannot constitute this. At birth there is pure psychological immediacy for which epistemological mediacy (essential for the constitution of any object) is implicit; developed human knowledge of objects is generated through the explication of these implicit factors, and is extended by growing experience and by description through explicit mediation: its objects remain "that-whats" only by their relation to psychological immediacy. Yet the objects of description (*e.g.*, physical objects) and the objects of acquaintance (*e.g.*, sense-data) are ill bedfellows, and their marriage has never been consummated. In what direction, then, shall we look for a satisfactory issue? The process of emending acquaintance by description leads towards the "thatless whats" of pure possibility which objectively is indistinguishable from nonentity. Such cannot be the true "emendation of the intellect". We must look for some form or factor or development of our apprehension that will lead us, not to nonentities, but to the real: and this is the end I have in view. It will be seen as we proceed that I find it in passage beyond the conception of knowledge as a subject-object relation, both for acquaintance and for description: with the former by reference to the nature of *ultimate* acquaintance as no contemplation of elementary objects but as the *enjoyment of action;* with the latter by the transcendence of objectivity in the *intuition* of the essence of the "thatless what" of perfected description as non-objective *action*, the ground of all possibility, and thus again superseding contemplation. But this end will not be advanced by the denial of the givenness of the data of outer sense on the ground that, as I have admitted, absolutely pure immediacy is empty.

[1] *Cf.* Russell's distinction of two kinds of "primitiveness" (*Our Knowledge of the External World*, pp. 69-70). The distinction thus drawn also allows for the illusions of sense to which we are subject; though I do not forget that Russell denies their possibility (*ibid.*, p. 85), and I am not sure that this denial involves no more than a difference of expression.

tion whether the objects of acquaintance are purely immediate or not may thus receive an Aristotelian quietus. What is of importance is to ask whether, taking the object of acquaintance as it is ostensibly given, it is of such a nature as to be capable of independent reality as an *ens in se*.

According to Russell[1] we are acquainted with: (*i*) outer sense-data; (*ii*) our own inner thoughts and feelings; (*iii*) remembered data of sense and inner consciousness; (*iv*) perhaps with the Self; and (*v*) certain universals. Of these objects of acquaintance perhaps I may be allowed to neglect the last three: universals because I am limiting my inquiry to singular existents; the Self because Russell himself appears to be very doubtful about it; and memories because what I shall have to say about the present data of sense and inner consciousness will apply to them *a fortiori*.

(*i*) Consider, then, our *outer sense-data* as objects of indubitable acquaintance. I do not consider the purity of their immediacy under an ultimate analysis, but their ostensible character as presented. Let it be granted that our knowledge of them *is* indubitable: this would appear to entail their reality; but what kind of a reality is a sense-datum? It is an *objective* entity, the reality of an "other" presented as *external* to the knower. This externality is signalized by the phrase "*outer* sense" as applied to the mode of our knowledge of sense-data. This is, indeed, the differentia of sense-experience as distinct from what Russell calls "self-consciousness" or "*inner* sense". Sense-data are externally presented; and since externality is a symmetrical relation, our knowledge of sense-data must be an apprehension of some entity or entities viewed externally, as they are *from outside*, and not as they are *in themselves*. If they are outside of us, we are outside of them, and thus cut off from their inner essence. The "red patch" is presented to us externally: we do not gather from this what "red" is *in itself*, what it is like to *be* "red" and not merely to be presented as red to another (or to the self in external perception). This, in fact, as I have already said, is the very source of our effort, never relaxed, to proceed beyond acquaintance to description, and thus to grasp the full nature of the objects that are presented for our acquaintance. The aim of knowledge-*about* is precisely to discover the inner nature of facts, to know "what makes them what they are".[2] That effort is largely fruitless because of its failure to take due

[1] *Problems of Philosophy*, pp. 73-81. Cf. *Our Knowledge of the External World*, pp. 70-75.

[2] James, *Principles of Psychology*, i, p. 221.

account of the special defect of outer apprehension that I have been emphasizing. For the descriptions, though more complex, or even more subtle and relatively adequate, are nevertheless descriptions of external objects still, *i.e.*, of " others " viewed externally and not as they are *in themselves*,[1] *i.e.*, as real. Do not let me be mistaken : I am not saying that knowledge fails because there are no things *other than* the knower, and distinct from him. We ought to make a serious effort to forget the opposition of idealism and realism that has for too long been a sort of monomania among philosophers and their audiences. No one really doubts, or even has reason to doubt, the reality of beings other than himself ; [2] the point I am now making is that the external objects of our acquaintance are disqualified for reality by the conditions of their apprehension : their *externality* to the self involves the externality of the self to them, and thus the externality of their presentation. *So far is it from being the case that objectivity is equivalent to reality, it is essentially disqualified by its externality.*[3] But this does not imply that the reality of a thing is its *mind*, for mind is only one of the actions constituting the *inseitas* of a real, and it is neither ubiquitous nor ultimate.

But, it may be objected, why should not a " qualitied contour " be real though it is not a thing-in-itself ? Why should we seek to go further than the overt data of experience ? I reply : we do not need to go beyond these data to establish our point ; we need not attempt to give *a priori* reasons for the denial of reality to sense-data ; for we are especially favourably situated with respect to one part of what appears as the objective world, *viz.*, what we call our " sense-organs ". These we not only contemplate externally as qualitied contours, but also enjoy as things-in-themselves. Thus we can say, and in a sense more

[1] The phrase " thing-in-itself " tends to become a *cliché* and to lose its primitive and vital significance. No one who held clearly in mind its ostensible connotation could even discuss the question whether physical objects are the things-in-themselves : they are too clearly things-from-outside. Nor could he take the thing-in-itself as no more than the permanent object that presents changing appearances, for no object, or thing-from-outside, can be a thing-in-itself.

[2] For to doubt is to be so far thwarted : if by another, then that other is at least as real as the self that doubts ; if by the self, then the self is finite, and the same conclusion follows. We may doubt the reality of things *external to us*, and with reason : we cannot doubt the reality of beings *other than* the self.

[3] Let me make it unambiguously clear that strict knowledge with objects is impossible, not because they are *other*, but because they are *external*. The subject cannot contemplate the inseity of the object. The *otherness* of " self " and " other " need in no wise conceal the inseity of the " other " from the " self ". In love the " self " *enjoys* the " other ".

à propos than Berkeley and Russell, that " a man need only open his eyes to see " that qualitied contours as such are lacking in reality. For *them* the assertion is no more than an analogical way of asserting self-evidence, but *I* am asserting that the very " seeing " of our eyes convinces us that they are not in themselves realized as objective qualitied contours. Sound empirical evidence is thus available even as we open our eyes to see. The eye is no mere *seen* object, an object for another's seeing, but an active *seeing* entity. Look at your eye in a mirror : it is a coloured superficies of some complexity. Suppose it dissected : still it presents to sight only varied coloured superficies. Take it as an infinite system or assemblage of such actually or potentially coloured superficies—still this is not the eye that *sees*, but only the *visible* eye. To discover the real eye we must turn away from the object that is seen, to the " same " eye as it is look*ing* at itself in the mirror. Now it is no object, at least so far as it functions efficiently : it is a *seeing* thing, a functioning *ens in se*. If it is present in vision at all it is as that expanse of visual space-time within which all particular visual objects are located. And evidently, this is the same eye that appears as object in the mirror, now appearing and now functioning, now object and now real.

The same is true of every part of the sentient body : the finger that is a tangible object to the adjacent thumb, in itself is no object but an acting entity ; not a touch*ed* but a touch*ing* ; not a qualitied contour but an *ens in se*. And so on.

Now there is no reason to suppose that the thing I call " my body " is different in any fundamental way from your body that I contemplate as a qualitied contour, nor your body from the other things that appear in objective acquaintance : that is, no fundamental *objective* difference. And the reason why I say that the object is not real is because it *does* nothing ; it does not even " appear " : it *is* an appearance. But the real that thus appears is *a thing that acts*, or a community of things that act ; and this is true even for those things whose activity does not include that of knowing. It may well be that many things that appear do not know ; that many things that know do not appear ; and that many things that act neither know nor appear, But nothing is real that does not in some way *function*. " We exist," says Sir William Hamilton, " only as we energize," and the principle may be fully generalized. The essence of any real, then, is the action that it enacts ; and by that I do not mean the motions that it suffers, the process in which it is involved, the objective changes that it exhibits in objective appearance. An action in itself is not temporal, though we may come to regard it as an en-

during action. We do so only in so far as the action *generates* time, and is dated by reference to the time that it generates from a non-temporal " present ". Sight *qua* sight is an eternal action, for though we may continue to see, the act of seeing in itself is not temporal. So we may believe that the action that constitutes the reality of a " material thing ", that appears as a variously qualitied spatio-temporal contour, is something that it eternally enacts, not as an object but as an *ens in se*.[1]

(*ii*) But we have acquaintance, it is said, not only with outer sense-data, but with our own *inner thoughts and feelings and desires*, and these at least would seem to avoid the objections I have been raising. Unfortunately, however, so set is the mind by its normal practical interests in objects to the contemplation of them, that it tends constantly to objectify what it apprehends even when it should be clear that this is neither external nor other. In this natural bias it is re-inforced by our habit of using abstract terms like " knowledge ", " thought ", " desire ", " conation ", " volition ", and the like, which erect as objective abstractions what are really not objects at all but actions: knowing, thinking, desiring, endeavouring, willing. To this we are also predisposed by the truly scandalous ambiguity of the term " object " in its modern Hydra-headed usage. Of course if *any*thing that is in *any* manner apprehended is to be styled " object " then knowledge must be exclusively a subject-object relation, and " objects " alone can be apprehended. So we find even so careful a writer as Russell speaking of " my seeing the sun " or " my desiring food " as " an object with which I am acquainted ",[2] when it is evident for the most superficial reflection that " seeing " and " desiring " are not " objects " of acquaintance at all when we are acquainted with them. And I think that Russell himself is conscious that this way of speaking is inappropriate with " the Self " " which has the thought or feeling ": he calls it the " person " acquainted with the " object ",[3] though even here one feels that the difficulty that he finds with reference to our reputed acquaintance with the Self [4] is very largely due to his inability,

[1] I shall return to these questions in the second part of this article. The distinction between an *ens in se* and an object is more fully discussed in my articles on " The Roots of Duality in Human Knowledge " (*Proc. Arist. Soc.*, 1937-1938, pp. 168-188) and " On Things in Themselves " (*Philosophy*, xiv, pp. 155-179).

[2] *Problems of Philosophy*, p. 77. [3] *Ibid.*, p. 79.

[4] Which within a couple of years he resolved by an unqualified denial. Cf. *Problems of Philosophy* (1912), pp. 78-80; *Our Knowledge of the External World* (1914), p. 74: " The bare subject, if it exists at all, is an inference, and is not part of the data ". In the interval he had evidently braced himself to the " rashness " of " speaking positively " on this " very difficult question ".

with Hume, to discover it as an " object ". But even with the thoughts, feelings, and desires, of the Self the same difficulty should have been apparent, and would, I think, be suggested, almost irresistibly, to anyone who discarded the abstract substantives by which we are apt to name these forms of the activity of the Self, and made use of the, at least more appropriate, gerunds: thinking, desiring, willing, etc.[1]

It is to Samuel Alexander that we most naturally look for a more discriminating account of our acquaintance with our own actions. So impressed was he, and so rightly, with Lloyd Morgan's emphasis upon the distinction of " -eds " and " -ings " that, avoiding the already ambiguous term " immediate experience ", he invented the humorously inappropriate, and therefore memorable, name of " enjoyment " for our acquaintance with certain of our own actions, reserving " contemplation " for our acquaintance with, or knowledge in general of, objects. There can, I think, be no doubt that Alexander's original account of this distinction is in intention at least unexceptionable: " Any experience whatever may be analysed into two distinct elements and their relation to one another . . . the act of mind or the awareness, and . . . the object of which it is aware; the relation between them is that they are together or compresent in the world which is thus so far experienced. . . . But the two terms are differently experienced. The one is experienced . . . as the act of experiencing, the other is that which is experienced. . . . I am aware of my awareness. . . . My awareness and my being aware of it are identical . . . the mind enjoys itself and contemplates its objects. The act of mind is an enjoyment; the object is contemplated." [2]

Thus contemplating an object involves also the enjoying of the contemplating: contemplating and enjoying contemplating are identical; for contemplating is an acting that exists as an enjoying. We enjoy the contemplating of objects, but we do not contemplate the enjoying of contemplating. We enjoy

[1] Though I admit that the case of the term " feelings ", that *is* in common use, shows the extent to which the grammatical usage that makes participles as such adjectival and drives us to the gerund, as well as the natural " radical objectivism " of the finite mind, may sidetrack us.

I need hardly say that I am not denying the utility of the abstract substantive in literature and common speech, or even in philosophical discourse where confusion is not likely to arise, or has been forestalled.

The objection raised applies equally to the use of the term " event " for an action: " Our sensation, which is a mental event consisting in our being aware of a sensible object . . . " (Russell, *Our Knowledge of the External World*, p. 76).

[2] *Space, Time, and Deity*, i, pp. 11-12.

contemplating. We must not, Alexander tells us, "regard the mind as contemplating itself"[1]: the acceptance of experience as a relation of "compresence" depends upon our taking the mind as something enjoyed and the object as something contemplated, and not both as objects of contemplation.[2] So far, in the main, so good. Contemplating is an action distinct from its object; but enjoying the contemplating is inseparable from the contemplating. Contemplating is conscious as an action as well as in respect of its object. Enjoyment, then, if it is acquaintance at all, is not acquaintance with an object but a certain *objectless realization;* and what I must insist upon is that we must be constantly on our guard against thinking of it as a form of knowledge differing from contemplation only in its "object", that object being an *action* instead of a *thing*. That we are liable to this fundamental confusion can be traced, as I have suggested, to the operation of one of those linguistic decoys against which recent philosophical movements have made us increasingly vigilant. It arises, I think, from the use of a transitive verb for an action that is essentially intransitive, and the attempt to escape between the horns by making it reflexive. "My awareness", says Alexander, "and my being aware of it are identical": [3] that is, contemplating an object is the same thing as enjoying the contemplating of the object; to enjoy, therefore, though it is grammatically transitive is epistemologically intransitive,—a fact that we conceal by saying that "I enjoy my contemplating", as if the contemplating were the reflex object of the enjoying. This confusion can be verbally avoided if we say: "we are consciously aware of objects" instead of: "we are aware of our awareness of objects", *i.e.*, "we enjoy our contemplation of objects"; but if we do so it becomes clear that to be aware and to be consciously aware are identical, so that, in relation to our own actions, enjoying is a form of knowledge indeed, but in a very special sense, and certainly not in the subject-object sense. It is also a form of *being*,[4] *viz.*, the conscious being of certain of our actions.

Such, then, I take to be the formal account of our acquaintance with, or consciousness of, our own actions given by Alexander. I have elsewhere expressed doubts concerning the application of the distinction by the author, naming especially his *jeu d'esprit*

[1] *Space, Time, and Deity*, p. 17. [2] *Loc. cit.* [3] *Ibid.*, p. 12.

[4] It will be seen in the issue that I do not myself limit "enjoying" to our enjoyment of "self", and thus also to a form of being. For in community with the "other" we also enjoy it: yet without ontological identity.

about " the angel's view ", which I do not altogether understand but which I feel may be of crucial importance ; and more certainly his account of mental or enjoyed space and time, which seems to be essential to his metaphysical system.[1] I must not occupy space unduly with these topics but it is instructive, I think, to reflect upon their implications for the theory of acquaintance by enjoyment. As I have said, Alexander emphasizes the identity of cognition with the general relation of " compresence " as depending on the view that the mind enjoys itself and contemplates its object. We have not two compresent objects, but an action and an object. The reputed " uniqueness " of the cognitive relation is thus transferred from the relation to the terms, one of which is an enjoyment and the other an object. But this distinction of terms is only possible in the knower's own perspective of things, enjoyment being as it were internal. For a higher being both terms would be objects. For an angel " my consciousness would be an object equally with the tree, and he would see my enjoyment compresent with the tree, much in the same way as I may see a tree compresent with the earth ".[2] But, of course, since both I and my object, the tree, would be objects to the angel he would not have my knowledge of the tree : he would miss our special relation of cognition because this requires the compresence of an enjoyment with an object. For him there would be compresent a mental object and a physical object.

I confess that I find this part of Alexander's doctrine at once bewildering and intriguing : *prima facie* it would seem to conflict with the account of enjoyment which (unlike contemplation which can be enjoyed) cannot be contemplated. But it is, of course, only the enjoyer himself who cannot contemplate his enjoyment : the angel could not contemplate his own angelic enjoyment, but he can contemplate the human enjoyment. To the form of such an objective enjoyment Alexander gives us no clue ; nor does he cite instances analogous from our own experience, in which we contemplate the enjoyments of lower beings, standing in relation to them as the " angel " stands to us. This seems a little odd in a thinker who regards philosophy as " empirical in method " (if not altogether so in content),[3] for presumably we have experience of lower beings better certified than our experience of " angels ". Yet, nevertheless, I think that we have here a metaphysical principle of central importance, and especially for those

[1] Cf. *Philosophy*, xiv, p. 157. The relevant passages in Alexander are : *Space, Time, and Deity*, i, pp. 19-20 ; bk. i, chs. 3 and 4 *passim*.

[2] *Space, Time, and Deity*, i, p. 19. [3] *Ibid.*, p. 4.

who, like myself, take the view that reality means action, and that every [1] real thing-in-itself is real as non-objective action yet is able to make objective appearance [2] to other real things, *i.e.*, as qualitied spatio-temporal contour. The " red " of the red-hot poker is thus the objective appearance of the action of the real " poker " : action that the poker enjoys appearing for another agent as contemplated object in space-time. The main difference between such a view and that of Alexander would then be that the space-time must " emerge " with the objectification of the action, and formally constitute it, and cannot belong to the action as enjoyed by the poker.[3] For Alexander finds reason to believe in a space and a time that are mental, *i.e.*, enjoyed, and not by us contemplated, though standing in relation to our contemplated space and time. This, I confess, is a conception that, at present, I find it impossible to accept. " That the mind as the experienced continuum of mental acts . . . is a time-series ", he tells us, " would be admitted on all hands " ; it " has Time in its very constitution " : [4] but however widely this may be admitted, I can even find it in my heart to deny the temporality of mental *action :* I do not think that an enjoyed act of mind is as such and in essence temporal, though its conditions may be contemplated as having been in time, because the time-series of events is generated by the co-operation of such actions.[5]

But whatever view we may take about mental time, when we consider the asserted spatiality of enjoyed mental acts our scepticism grows. How can it be said that the " mind enjoys itself spatially ", that " mind is spatial like matter " ? [6] Even with the warning that mind enjoys itself, not in contemplated space, but in " enjoyed space ", the saying is too hard : unless,

[1] And not excepting the Real whose objective expression for us constitutes creation.

[2] So that to " appear " is not merely to present a selection of characters or parts but, while remaining in itself an enjoyment, to become the object of contemplation ; while remaining an action, to be contemplated as a qualitied contour of space-time.

[3] It is the objectified response of the contemplator to the objectified action of the " other ", or rather, what objectifies that action. Thus the object contemplated owes part of its nature, roughly, the formal elements, to the contemplating " self " ; the " material " and differentiating elements, to the appearing " other ". I shall return to this subject in greater precision in Part II.

[4] *Space, Time, and Deity*, i, p. 94.

[5] What I have said above about the eternity of the act of seeing *qua* seeing may be generalized. *Cf.* also Part II, below, under " Time ".

[6] *Space, Time, and Deity*, i, p. 97.

indeed, "enjoyed space" were not, after all, an extent or volume, but something else.[1] Alexander's explicit statements, however, show the contrary: "My mind is for me . . . spread out or voluminous in its enjoyment. Within this vague . . . volume the separate and salient mental acts or processes stand out as having position and direction."[2] As against this description, however, it seems to me that voluminousness is essentially an object of contemplation, only to be enjoyed in the sense that the contemplation we enjoy is a contemplation of voluminous objects. If we call this a "voluminous contemplation" it will be well to substitute the gerund for the abstract substantive, and to ask whether the contemplating that we enjoy is voluminous. If it is, we can go on to say that our enjoyment of that voluminous contemplating is a voluminous enjoying; if, as I think, it is not, we are bound to reject the conception of mental space—except, perhaps, in the derived sense analogous to that allowed in the case of mental time. Just as we date our enjoyment by reference to the time generated by our action, so we locate it by reference to the distance generated by our action.[3]

Let me now attempt to bring together the threads of this part of my argument, and to lay down what I take to be the essential nature of knowledge, and of reality, and of the relations holding between them. Knowledge, I have suggested, is not generally definable as a subject-object relation: so to define it necessarily entails a general scepticism; for, on the one hand, the distinction of knowledge and opinion or belief at least involves the assured *truth* of the former, so that what is genuinely known must be apprehended in its real character as it is *in itself*[4] as a functioning existent. Yet, on the other hand, from its very nature no object *as such can* be so apprehended: *qua object* it has no inseity; it is an *ens ab extra* and no *ens in se;* it is an ambiguous appearance of a real, or congress of reals, and not these as they are in themselves. Objects *qua* objects do not act: all action attributed

[1] For a suggestion of space in itself as action rather than extent we must have recourse to the Extension of Spinoza, not regarded as an *Attribute* of Substance, *i.e.*, as conceived by the intellect "*as if* constituting the essence of Substance", but as *actually* constituting that essence. But this is an *in*conception rather than a conception, though that is not Spinoza's view.

[2] *Loc. cit.* [3] *Cf.* Part II, under "Space" and "Time".

[4] Has any more desperately sceptical assertion ever been made than the Kantian commonplace that we have *no knowledge of things-in-themselves?* If the phrase had not become a mere *cliché* it would be apparent that there can be no *knowledge except* of things-in-themselves, *i.e.*, of *reals.* If we have not this, we have nothing! For *knowledge* must be knowledge of the *real.*

to them has been imputed to them, or conveyed through them, and can always, therefore, be denied by the sceptic. Even where objects are apprehended as "moments" of *process*, this process is an objective *passage* not apprehended from within as *agency:* the agency is imputed, or conveyed, and again by the sceptic denied. If knowledge terminates in objects *qua* objects it can never be true, and hence never knowledge. We must look, therefore, to a form of apprehension that terminates in reals, in *entia aeterna* and not *entia externa*, in inner actions and not superficial appearances. We must look, in Alexander's words, not to contemplations but to enjoyments.

But, it may be suggested, may it not be possible to distinguish in the objects between that which makes them external, their spatio-temporality, and that which signalizes their genuine reality, their qualitative content? If so, the former might then be taken as signifying only their relation to our localized and dated incidence, while the latter would remain as their definite positive claim to reality. I reply: *on the plane of objectivity* the attempted distinction is idle, whether we apply formal principles or rely upon experience; for, in the one case, the qualitative content must vanish with the abstraction of its spatio-temporality, *e.g.*, an enduring red area must cease to be red if it ceases to be an area and to endure; and, in the other case, it is the remnants of spatio-temporality in our empirical data that secure their objectivity, and thus so far conceal their reality. I say "conceal" their reality, for in spite of what I have just said I must agree that we are not compelled to suppose that the quality of the object, or even its spatio-temporality, is unambiguously unreal: it remains possible to take the quality as the spatio-temporalized appearance of the real action of the "other" with the "self", and the spatio-temporality as the qualitied appearance of the real action of the "self" with the "other". A "red" that is not an "enduring red area" is not "red", and an "enduring area" that is not qualitied is neither extended nor enduring. Empty space-time = 0. Yet if we allow ourselves the liberty that we take when, *e.g.*, we speak of "blushing modesty" or "keen-eyed vigilance" we may well say that the red of the enduring red area is the spatio-temporalized appearance of a "red-action".[1] And this, I think, is what Leibniz meant when

[1] It is to be noted that "red" is not here the *adjective* of "action" but is in *apposition*. So also I read the analogous phrases I have quoted, though common usage tends to read the gerund as participle, and so proceeds (as, explicitly, in the second example) to the adjective under cover of a figure of speech.

he described sense-qualities as "occult". In relation to them we are in the position of Alexander's "angel": we contemplate their enjoyed agency as objective quality. We may even assume a more exalted analogy and compare ourselves with the "God" of Spinoza: the agency of the "self" co-operates with that of the "other" as joint creator of the qualitied spatio-temporal object, as *Deus sive Natura* (*respectu intellectus*) eternally creates its own actualization. When Spinoza identifies *Natura naturans* with *Natura naturata*, a creat*ing* with a creat*ed*, he is in fact identifying *actio* with *actus*, and it is his further identification of *actus* with "object" that is the root of his often-imputed, and generally-decried, "intellectualism".[1]

It is important to notice that the nature of this metaphysical derivation of objects from agency renders wholly futile any request or attempt to describe its steps or "mechanism" as if it were an objective process.[2] Only unusually callow positivist minds will be tempted to interpret this as meaning that it is advanced as a mere dogma. It has, in fact, stubborn empirical foundations in our apprehension of objects and agents; and if it is not a uniformly "hard datum" throughout the whole range of these, at least it is "softest" at the lower extremity of the hierarchy, where we can most easily take the object for that and nothing more; and "hardest" where we are dealing with entities fully co-ordinate with ourselves, where no room is left for doubt that objects are *informed* with agency. I have said already that

[1] Thus for *Natura* the identity of *actio* and *actus*-object is complete and unambiguous, for "self" and "other" are one, and the "object" is the self-expression of the identity; but for the finite "self" the "other" is complementary and constitutes a given "field" within which liberty is achieved through "community" (*cf.* "The Moral Good", in *Hume and Present Day Problems*, Arist. Soc. Supp. Vol. xviii, pp. 144-178). Thus the identity must here remain occult because it is the "self" that apprehends the ambiguous "object" that is the expression, not of itself alone, but of its congress with the "other". The important metaphysical question then outstanding would be the character to be imputed to *Natura*: whether, and in what sense, *e.g.*, Extension may be said to be its Attribute. Spinoza takes this to be one *meaning* of "actualization", and holds that without some such Attribute no difference could be perceived by the intellect between *actio* and *actus*. Yet he denies that it is the *sole* meaning: there are infinite ways in which *actio pura* actualizes itself. In the case of *time* he is more explicit: *Natura naturans* is eternal,—constitutes eternity; and thus *Natura naturata* must be conceived *sub specie aeternitatis*.

[2] Even the scientific "reduction" of colour or sound to oscillatory spatio-temporal processes, which points analogically towards such derivation, can exhibit no objective steps of process: yet a spatio-temporal process, however minute or ample, is as far from agency as the colour or sound derived from it. It differs *toto coelo* from an action generating space and time yet in itself remaining internal and eternal.

"no one really doubts, or even has reason to doubt, the reality of beings other than himself": in so speaking I was thinking especially of our assurance of the reality (*i.e.*, the agency) of those with whom we are in any relations of "community". I do not doubt the real agency of the enemy who opposes me with implacable enmity, of the alien mind whose interests are so remote that I can only meet him with generous toleration, of the *alter ego* who engages my friendship and responds to it; least of all does the lover doubt the reality and agency of his beloved.[1] Scepticism mainly concerns things conceived as objects of purely contemplative interest, or as so much objective "junk" happening to occupy our bodily environment, but not in use. For the working navigator the stars and planets are indisputably real, and for the craftsman the tools and materials with, and within, which he functions are beyond dubiety. Is it not evident that our belief is a function of what we sometimes call "sympathy" with the "other", *i.e.*, of our entrance into, and our enjoyment of, the agency of the "other"?

I am well aware, of course, that thus to speak of enjoying the enjoyment of the "other" has been condemned as self-contradictory, as hyperbole, or at best self-deception arising from passion or mysticism;[2] but I believe that these judgments are based upon an uncritical acceptance of that ultimacy of objectivity that it has been my chief aim to discredit. It seems to me that we *do* have experience of identity of enjoyment with the "other", though we do *not* sense its sensations, feel its feelings, or act upon its impulses. Sensations, feelings, and impulses are objects of contemplation, and not what I have said we may share, *viz.*, enjoyings. I go further and say that if we could not thus enjoy the enjoyings of the "other", the "other" would remain dubious object, and not instrument, enemy, alien, friend, or lover, acting in "community" with the "self".[3]

I suggest, then, that we have here a stubborn empirical fact that taken in its totality is the hardest of hard data: the craftsman in the very act of using his tool cannot doubt its reality because he enjoys his agency in the agency of the tool. When he puts it aside and philosophizes about it as a mere object he may waver in his belief as he contemplates it; the assurance that we have

[1] *Cf.* "The Moral Good" (*loc. cit.*).

[2] *Cf.* Pringle Pattison, *Man's Place in the Cosmos*, pp. 124-127.

[3] This makes clear what I have already emphasized: that *otherness* is no bar to knowledge, but only *externality*. Contemplation fails, but *enjoyment* carries us into the inseity of the "other" in the measure of our hierarchical relativity.

of the reality of the "other" is indeed our enjoyment of its active reality. Our enjoyment of our own action is our certainty of our own reality: the principle need receive no Cartesian limitation to thinking; but neither must it receive a solipsistic limitation as if we were confined in our enjoyments to our own actions. In love we enjoy the action of the beloved *as if* it were our own; and only so is our love genuine. As soon as we lose this identity of enjoyed *agency* and begin to use the "other" as an amusing *object*, love is degraded to play; as soon as we begin to find in the amusing object an agency with which we can co-operate in a mutual active enjoyment, play is elevated to serious love.[1] And this, I think, is the principle that must govern our interpretation of all that "well-founded opinion" that passes under the equivocal name of "'knowledge' by contemplation": it trembles upon the brink of illumination. "We are green in Heaven's eyes" when we take the object *qua* object as real in its own right; but equally when we take it as mere *Schein*. The quest is for those who elevate contemplation into love, and playful isolation into "community". To look, as "radical objectivists" have always done, to the unprejudiced contemplation of objects as the sole and satisfactory way of knowledge, or even, in itself, as a way at all, is to suffer from blindness "too blind to have desire to see". If we are to apprehend the reality, of which the "object" is but "occult appearance"[2] we must throw in our lot with the "other", so that it may convince us of its agency by mutually enjoyed co-operation. This is art; this is morality; this is knowledge indeed.

(*To be concluded.*)

[1] The same, *mutatis mutandis*, is of course true of personal hate also. Morality is not quite the simple thing that might otherwise be supposed.

[2] The self-contradictory phrase is used to indicate the demand made on the "self" by the "other", and on the "other" by the "self".

IV.—ARE NECESSARY PROPOSITIONS REALLY VERBAL?

By Norman Malcolm.

When people talk about the nature of necessary propositions there is a tendency to say that necessary propositions describe relations between objects of a special sort, *i.e.* abstract objects, *i.e.* universals, numbers, propositions. There is also a tendency to say that necessary propositions, or at least some of them, describe the most general characteristics of physical objects, minds, space, time, colour, etc.; or that they describe the structure of reality, or the most fundamental features of the world, and so on. But there is another common tendency opposed to both of these, which is the one I wish to discuss, and which may be roughly indicated by the words "Necessary propositions are really verbal". An illustration of this latter way of talking is Carnap's saying that necessary modal statements like "A is impossible" (where A is a proposition and "impossible" means logically impossible) are quasi-syntactical sentences and are *translatable* into proper syntactical sentences like "'A' is contradictory" (where "contradictory" is a property of an expression).[1] It looks as if he were saying, in other words, that a necessary statement about a proposition is translatable into a *contingent* statement about the *sentence* which expresses the proposition. And Carnap apparently holds that *all* necessary statements are translatable into corresponding contingent verbal statements, for he says, "Translatability into the formal mode of speech constitutes the touchstone . . . for all sentences which do not belong to the language of any one of the empirical sciences".[2]

Wittgenstein has said in lectures that statements such as "the colours green and blue can't be in the same place simultaneously", and "3 × 18 inches won't go into 3 feet" are *rules of grammar*. Wisdom has said [3] that necessary statements are "purely verbal", that "they are purely about the use of the expressions they connect". Ayer said that all necessary

[1] The *Logical Syntax of Language*, p. 253. [2] *Ibid.*, p. 313.
[3] "Metaphysics and Verification," Mind, vol. 47, p. 463, footnote.

propositions are analytic [1] and remarked of analytic propositions that "they simply record our determinations to use words in a certain fashion",[2] or again that "they call attention to linguistic usages".[3]

In criticising Ayer's statements, Broad said, "If an analytic proposition states that the person who records it intends to use certain words in certain ways, it evidently makes a statement about the present experiences of the speaker and about his future behaviour . . . The analytic proposition is therefore quite plainly synthetic and empirical." [4] On the other hand, "If an analytic proposition" calls attention to linguistic usages "it states an alleged matter of fact about the majority of writers and speakers of the language which the speaker is using . . . The analytic proposition is therefore quite plainly synthetic and empirical."

Feeling that such a view was too absurd, Broad suggested as a "charitable" account of what Ayer *really* meant, the following theory: There are no *a priori* propositions at all really, but there are certain propositions which *seem* to be *a priori* propositions, and these "ostensibly *a priori* propositions are really synthetic empirical propositions of a certain kind, *i.e.* announcements by the speaker of his present intention to use certain words and phrases in certain ways, or statements about the current usage of certain words and phrases in a certain language".[5] Thus Broad represented Ayer as making the paradoxical claim: Necessary propositions are not really necessary.

Feeling discontented with this account of what he meant, Ayer said in a later paper,[6] " . . . those who take a conventionalist view of *a priori* propositions do not mean to hold the theory that what are commonly said to be *a priori* propositions are really empirical propositions about the way words are used . . . It might be held that they are not, in the ordinary sense of the term, propositions at all."

"I suggest that this is what is really being maintained by conventionalists. I think that our view must be that what are called *a priori* propositions do not describe how words are actually used but merely prescribe how words are to be used. They make no statement whose truth can be accepted or denied. They merely lay down a rule which can be followed or disobeyed."

[1] *Language, Truth, and Logic*, p. 113.
[2] *Ibid.*, p. 114.
[3] *Ibid.*, p. 105.
[4] "Are there Synthetic *A Priori* Truths?" *Aris. Soc. Suppl. Vol.* xv, p. 107.
[5] *Ibid.*, p. 109.
[6] "Truth by Convention", *Analysis*, vol. 4, nos. 2 and 3, pp. 19-20.

Thus Ayer dodged the paradox, "Necessary propositions are not really necessary", by embracing the paradox, "Necessary propositions are not really propositions". An important effect of the linguistic view's seeming to get itself into such absurdities is to drive other philosophers to react against it by declaring for the view that necessary statements are really factual statements about reality. For example, another member of the symposium with Ayer,[1] after noting that "I cannot be in London and Birmingham at the same time" is not a grammatical rule like the one which forbids one to say "I seen him", nor a command like "Shut the door", says, "I have come to the conclusion that attempts to represent sentences like 'I cannot be in London and Birmingham at the same time' as statements of linguistic conventions, or as stating nothing at all, are not satisfactory; and I have committed myself to the view that the sentence states a matter of fact about the world, about me and London and Birmingham".[2]

Now what is the explanation of this curious state of affairs? What is it that inclines philosophers to say such obviously queer things as "Necessary propositions are not really necessary, but are contingent, empirical propositions about the use of words"; or "Necessary propositions are not really propositions, but are rules for the use of words, or perhaps commands to use words in a certain way"? If we see what leads philosophers to say these things and what leads other philosophers to deny them, we shall have seen to what extent it is illuminating and to what extent misleading to say that necessary propositions are really verbal.

Let us begin by asking: How do we get to know necessary truths? How do we know, for example, that if fleas are smaller than monkeys, then monkeys must be larger than fleas? A stock reply is, "Because it is self-evident".[3] But this is a useless sort of reply; it leaves us where we were. In order to show how useless it is we call attention to the fact that in certain circumstances we apply the expression "self-evident" to *empirical* propositions. Suppose, for example, that you and a lot of other people were in a room the ceiling of which sagged precariously. You might say, "If that ceiling collapses somebody

[1] C. H. Whitely, *Analysis*, vol. 4, nos. 2 and 3, pp. 22-28.

[2] *Ibid.*, pp. 26-27.

[3] *E.g.* Broad, *op. cit.*, p. 102. ". . . we may say that a person 'knows a certain proposition *a priori*' if either it is self-evident to him or he actually sees it to be logically entailed by premises all of which are self-evident to him".

will get hurt". If someone said, "How do you know?" you might quite naturally reply, "Don't be silly! It's self-evident." This merely brings out the fact that there is a use of "self-evident" in which empirical propositions are said to be self-evident. The person who wishes to distinguish necessary truths by saying that they are the ones which are self-evident, will probably now say, "When I said that we know that if fleas are smaller than monkeys then monkeys must be larger than fleas because it is self-evident, I did not mean in that empirical sense of 'self-evident' but in another sense." We then ask, "*What* other sense?" He may reply, "In the sense in which three times three equals nine, All S is P entails No S is not-P, Nothing can be both red and green at the same time, are all self-evident". We shall then say, "But when we asked, How do you know that if fleas are smaller than monkeys, then monkeys must be larger than fleas?, we were really asking a question about *all a priori* truths. We were asking with respect to any necessary truth whatsoever, how do we know that it is true? For you to reply in the way you did simply amounts to your saying that we know *a priori* truths in the way we know them."

Thus we see that the reply that the way we know necessary truths is through their being self-evident, although true, is non-informative, says nothing. But not only is it a useless answer; it is also a harmful one. For it gives us a misleading picture of the way in which we get to know necessary truths. It gives us the idea that we find out, for example, whether a given proposition p entails another given proposition q, by holding the propositions before the mind's eye, submitting them to the searching gaze of that wonderful faculty, the intellect. We tend to have the idea that the process of finding out whether one proposition entails another is like the process of holding an electric bulb before our eyes and looking closely to see whether or not the filament is broken.

In order to see how misleading is this notion that the way we get to know the truth of necessary propositions is by *inspecting* them, we must see that we find out necessary truths in the same way that we find out the empirical truth that if you suddenly jab a man with a pin he will jump. It is by observing the way people react in certain circumstances that we learn that a man will jump if you jab him with a pin; and, likewise, it is by observing how people use expressions in certain circumstances that we learn necessary truths.

How does a person come to know the necessary truth that A is larger than B entails B is smaller than A? To answer this

let us imagine the following case: A child has learned most of the English language but has never heard or read or seen anyone apply the expressions "smaller than" and "larger than", or any synonymous expressions. Then one day he finds himself among a group of adults who are busy measuring objects around them and jotting down in a book data about the relative size of the objects. The child observes, for example, that Smith says to Jones, after applying a tape measure, "The bureau is larger than the desk", and that Jones writes down in a book, "The desk is smaller than the bureau". Smith then looks at what Jones has written and says, "Yes, that's right". Smith does some more measuring and calls out, "The red curtain is smaller than the brown one". Jones has not been paying attention and a moment later asks, "Did you say that the brown one is smaller?" Smith replies, "No, I said that the brown one is larger". Whereupon Jones writes in his book, "The red curtain is smaller than the brown one". The child also observes that when Jones is checking over the data in his book he says, "Something wrong here! I have it down that the fireplace in the library is larger than the one in the parlour, and also that the parlour fireplace is larger than the library one." Smith says, "It should be that the one in the library is larger. I remember saying that the parlour fireplace was smaller."

Through observing quite a lot of such talk the child gradually acquires the use of the expressions "larger than", "smaller than". He notices that Smith and Jones and other people too, use "A is smaller than B" and "B is larger than A", *interchangeably*. He notices that when a person has said that A is larger than B he does not object to being quoted as having said that B is smaller than A, and *vice versa;* while he does act as if what he said had been denied if some one says that B is *not* smaller than A. In noticing these characteristics of peoples' use of these expressions the child is *learning the necessary truth* that B is smaller than A follows from A is larger than B, and, *vice versa*, is learning the necessary truth that these propositions are equivalent.

In order to bring out more clearly how it is that the question as to whether one proposition entails another is a question as to how certain words are used, let us consider some cases which have a certain philosophical interest. Take the question, Does "I know that p" entail "I believe that p"? Some philosophers have been inclined to suppose that knowledge can be correctly analysed as true belief, *i.e.* that "I know that p" is equivalent to "I believe that p and p is true". One way of testing this

supposition is to ask, Does "I know that *p*" entail "I believe that *p*"? This is how we answer the question. We notice, for example, that people sometimes say, "No, I don't *believe* he did it; I *know* he did it". This shows that "know" is used in some circumstances in such a way that "I know that *p*" does *not* entail "I believe that *p*", but, on the contrary, entails "I do not believe that *p*". We may notice further that there is another use of "know" in which "I know that *p*" does *not* entail "*p* is true", *e.g.* when we say "I just *knew* something terrible would happen to him, but his luck was with him and he came through without a scratch". In this use of "know" it is equivalent to "feel sure", and we can observe that nobody would normally say, "I feel sure that I've forgotten something, but I don't believe I've forgotten anything". That is, anyone who said, "Jones feels sure (just *knows*) that Madge is guilty", would act as if his statement had been denied if some one retorted, "Jones doesn't believe any such thing". This shows that there is a use of "know" in which "I just *know* that *p*" entails "I believe that *p*". This is a *part* of the procedure we should have to go through in order to answer the philosopher's question, Does "I know that *p*" entail "I believe that *p*"? The whole procedure consists in noting that in certain circumstances people would *say* this and in other circumstances they would *say* that, and so on.

Take the question, Does "Smith has a pain" entail "Smith is aware of a pain"? Some philosophers have been tempted to deny that such an entailment holds, because they wanted to say that "awareness is of such a nature that nothing which is an object of awareness *depends* for *its existence* upon its being an object of awareness". Since they wished to say that when Smith is aware of a pain his pain is "an object of awareness", they felt compelled to maintain that Smith's pain could exist without Smith's being aware of it. The question may be answered in the following way: Suppose that two persons, A and B, are watching Smith. A says to B, "It looks to me as if Smith has a pain in his foot". B says, "Let's ask him". B asks Smith, "Does your foot hurt?" Smith says, "No". Suppose A said, "Oh, maybe there is a pain in your foot anyway, even if it doesn't hurt". Shouldn't we say that this is an absurd statement, that it does not make sense? Or suppose A said, "Maybe it hurts, but you are not aware of it". This again is something which would never be said. The circumstances in which we should say that Smith has a pain in his foot *are* just the circumstances in which we should say that Smith's foot hurts, or that

Smith is suffering from a painful foot, or that Smith is aware of a pain in his foot. If in ordinary discourse anyone made the statement, "Smith has a dreadul pain in his foot but he isn't aware of it", we should not know what to make of it, unless it was clear from the circumstances that it was intended as a joke. And so, not by peering into the nature of pain or of awareness, but by pointing out certain facts about what we *say*, about our use of these expressions, we show the philosopher the necessary truth that "Smith has a pain" entails "Smith is aware of a pain".

Let us take one more case. There is a very great inclination among philosophers to suppose that it can never be *certain* that a contingent physical-object statement is true, but that at best it can only be highly *probable*. Thus certain philosophers would be inclined to say that "p is a contingent physical-object statement" entails "It is not absolutely certain that p is true". It is very easy to show that these philosophers are wrong.[1] For example, it is absolutely certain that no man can chin himself 6,000,000 times in succession without rest. Our philosophers may say, "No, it is only immensely probable". But suppose that the greatest number of times that even the strongest and most supple of men has chinned himself is, say, 150 times. We should say that it is *very* probable that no man will ever chin himself 500 times. We should say that it is immensely probable that no man will chin himself 1000 times. We should say that it is absolutely *certain* that no man will chin himself 10,000 times, not to mention 6,000,000 times. This is *the way we use the expressions* "probable", "highly probable", "certain", "absolutely certain", etc. And by calling attention to the *actual usage* of these expressions we show the philosopher that "p is a contingent physical-object statement" does *not* entail "It is not absolutely certain that p is true".

The fact that a philosophical question of the form Does p entail q? is to be answered by an examination of *what people would say*, of our actual use of the expressions in question, and in no other way, has led people to say, "Philosophy is really grammar". But our concern now is not with this statement; our concern is to make it clear that the way to answer questions of the form, Does p entail q?, is by finding out certain empirical facts about the way we use words. In other words, it is a matter to be settled by *the eyes and the ears* (Wisdom's phrase) and not by some mysterious intuitive act of the intellect, just as the

[1] Although it is very difficult to explain what it is that makes them say this, in such a way that they no longer feel inclined to say it.

question, "How does a man behave when jabbed with a pin?" is a matter to be settled by the eyes and the ears.

It may be objected: "Oh, yes, it is true that the way we *find out* whether one proposition entails another is by learning something about the way certain expressions are used. But this is only the *way* we *find out*. The fact which we find out is something over and above the fact that the expressions are used in the way they are." What such an objector is doing is comparing the case of finding out whether two propositions are equivalent, say, with the case of finding out whether your watch is running. You find out whether your watch is running by listening for its ticking. But, of course, you may also find out by watching the hands to see if they move, or by taking the cover off the back and looking to see whether the wheels go round. We say that the fact that your watch is running certainly is something over and above the fact that you hear its ticking—that your hearing its ticking is only one of several *ways* we have of finding out whether it is running. But notice how different is the case of the equivalent propositions! Is there any way of finding out whether some two propositions are equivalent except by finding out whether some two expressions are used interchangeably? There is no process called "finding that some two propositions are equivalent *without* finding that some two expressions are used interchangeably"—because this is a *self-contradictory* expression. This shows how misleading is the analogy of the watch.

There is an inclination to suppose that sentences designate propositions in the way licence numbers designate automobiles. If two automobiles had the same licence number we might say that that licence number was ambiguous. And if some one asked, "Did the car with licence number 5640 have a flat tyre when you inspected it?" it might be replied, "Which car? There are two by that number which we inspected." Likewise, to the question, "Does 'Jones' horse finished third' entail 'A male animal finished third'"? it might be replied: "The sentence 'Jones' horse finished third' is ambiguous,[1] stands for two propositions. Which one do you mean?" We tend to suppose that these cases are exactly similar. "Tell me *which* car you mean and then I will find out whether it had a flat tyre." "Tell me *which* proposition you mean and then I will see whether the entailment in question holds." But there is a vast difference

[1] "Horse" is sometimes used to mean "equine, male or female", and sometimes used to mean "male equine".

between these two cases and it is this: *I can tell you which proposition I mean only by telling you whether the entailment in question holds*, while I can tell you which car I mean without telling you whether it had a flat tyre. I can tell you which proposition is meant only by telling you whether it entails "A male animal finished third". That is, to tell you which proposition is meant is to explain how the sentence, "Jones' horse finished third", is being used. If it is being used in such a way that the person who used it will act as if what he said was being denied if some one says, "A *mare* finished third", then the entailment *does* hold; while if he would reply, "Yes, Jones' horse is a mare", then the entailment does *not* hold. This is what we *call* the entailment's holding or not holding. We tend to suppose that we clear up the ambiguity and *then* we see whether the entailment holds. Whereas what is actually the case is that clearing up the ambiguity *is* seeing whether the entailment holds.[1]

Philosophers and logicians have the idea that when a question as to whether one statement entails another arises, verbal considerations enter only because of ambiguity, and that the *real* question is not a verbal one, but one to be settled by the intellect's fixing its gaze upon the propositions, *after* the ambiguity has been cleared up. But our point might be expressed like this: Clearing up the ambiguity of a statement *consists* in showing what it means, and this *consists* in showing how it is used, and this *consists* in showing what it entails. The procedure of seeing what a statement entails, the meaning of which is not clear, is not divisible into *two* processes—first, getting clear about its meaning, and second, seeing what it entails. It is the *single* process of getting clear about the meaning of the statement. And to get clear about its meaning is to find out *how* it is being used. And this is a matter to be settled by the eyes and ears—by observing how the person who used it would react or what he would say if somebody else said something of a certain sort, and so on. And so by this procedure we learn necessary truths.

So what the logician knows when he knows that "Some S is P is equivalent to Some P is S" is a law of logic, is a lot of facts of this sort; *e.g.* nobody would say that "Some of the nickels found in the slot machine were lead" and would at the same time deny that "Some lead nickels were found in the slot machine"; or would be surprised or protest if somebody quoted him as saying the latter, and so on. And what the logician knows in knowing that $p \equiv \sim(\sim p)$ is a law of logic, is a lot of

[1] This very important point was made clear to me by Wisdom, although I do not know whether I have made it clear here.

facts of this sort: *e.g.* that any educated person who, in the witness box, has used the sentence, "The accused man was kind to his wife", when asked to repeat his testimony will perhaps use the sentence, "It is not the case that the accused man was not kind to his wife", and *vice versa*. Of course, many uneducated people use double negation as *negation*, *e.g.* they say "There ain't nobody here". But what *makes* the above a logical law, what makes the logician *right* in saying that it is a logical law, is that nearly all educated people use double negative expressions in the way they use affirmative expressions. We can quite well imagine that our practice in this respect should gradually alter until everybody used double negative expressions in the way the uneducated minority now use them. And if this happened we should then say that $\sim(\sim p) = \sim p$ was a law of logic. What the logician knows in knowing that such and such is a law of logic is a vast number of facts about the way people use certain expressions. From noticing this people have been led to say "The logician is really a grammarian".

One is inclined to say, "But if what the logician knows when he knows that $p \equiv \sim(\sim p)$ is a law of logic, is a great number of facts about the way people use double negative and affirmative expressions, then is it not the case that necessary propositions are really empirical propositions about the way people happen to use expressions?" This is how the temptation arises to say that necessary propositions are not really necessary. And philosophers have asked whether "$2 + 2 = 4$" *means* "In modern English the sign '$2 + 2$' can be replaced in suitable contexts by the sign '4' without alteration of empirical meaning".[1]

One way of treating this puzzle is to point out that we *use* the expression "$2 + 2 = 4$" *in calculating*, and when in doing a calculation you mutter to yourself "2 and 2 is 4", you are obviously not *asserting* "In modern English the sign '$2 + 2$' can be replaced by the sign '4', etc." That is to say, when you use the mathematical expression in a calculation you are not making a statement about some language habits of English people or other people. You are *using it in a calculation*. We might express it this way: What *justifies* you in putting down $2 + 2 = 4$ in your calculation is a certain fact about the way everybody uses the expressions "$2 + 2$" and "4". But you do not *assert* this fact when in doing the calculation you write down or mutter to yourself "$2 + 2 = 4$". In other words, in these circumstances you are not using "$2 + 2 = 4$" to *describe a fact;* you

[1] *E.g.* M. Black, *Analysis*, vol. 4, nos. 2 and 3, p. 32.

are letting it play a rôle in a calculation. That is why it is wrong to say that a necessary proposition is really an empirical proposition describing how people use words. For when we use necessary statements in ordinary life we do not *use them* as *descriptive* statements.

It may help to bring out the point if we consider the following case: Suppose that some one who does not know who Lord Russell is, but who does know who Bertrand Russell is, were to ask you, "Is Lord Russell any relation to Mr. Bertrand Russell"? You might reply, "Lord Russell and Mr. Bertrand Russell are one and the same person". Now is your statement a verbal one, a statement about how certain words are used? As we ordinarily use these expressions we should say that it is *not* a verbal statement. But what *justifies* you in saying what you said is the fact that the words "Lord Russell" and "Mr. Bertrand Russell" are names of one and the same person—which, we might say, is a fact about words. This shows how it can be the case that when you make a statement, the fact which makes your statement correct may be a fact about words, although your statement would not be said to be expressing a fact about words, would not be said to be a verbal statement. Thus it is that when a man says, "My wife said that she would either be at home or at Mrs. Brown's. She isn't at home so we'll be able to find her at Mrs. Brown's," he is using a necessary statement as a rule of inference, is using it to make a deduction, and not to *describe* how people use statements of the form, "Either p or q and not-p". But what justifies him in using it as a rule of inference is a certain fact about the way people do use statements of that form. Closely analogous is the case of when you are playing bridge and say to yourself, "Oh, my! Two of hearts has been bid, so if I'm going to bid clubs I'll have to bid *three*". When you say this to yourself you are not *asserting* "It is a practice among bridge players to treat a bid of two in hearts as higher than a bid of two in clubs". Nor are you *asserting* that it is a rule of bridge. You are *using* the rule in *playing* bridge. But what justifies you in saying this, what makes your statement *correct*, is the fact that there is this common practice among bridge players.

So if some one says to us, "Is it not the case that necessary statements are really empirical statements about how words are used?" we reply in the following way: If you are asking whether necessary statements are *really* empirical statements then the answer is, Of course not! Arithmetical statements, valid syllogisms, etc., are what we *call* necessary statements, and what

we do not *call* empirical statements. Necessary statements are not used to describe empirical facts, but are used in making inferences, or in calculating. Nevertheless, the statement, "Necessary propositions are really empirical propositions describing how words are used", is an *illuminating* statement.[1] For it throws light on the nature of necessary propositions. It helps to dispel the illusion that necessary statements are descriptive statements which describe facts of a peculiar sort, *i.e.* facts about abstract objects, or facts about the structure of the universe, about the nature of colour, space, time, and so on. It helps to show that necessary statements are *based*, so to speak, on empirical facts about the way people use words; and that one learns necessary truths by learning these plain, unexciting facts, and not by a mysterious act of intuition. It helps to show how misleading it is to divide *facts* into two *sorts*, necessary facts and empirical facts; to show that there are empirical facts and, we might say, *nothing else*.[2]

When philosophers have suggested that necessary propositions are really verbal, people have constantly made the following objection: "We say that 'two plus two equals four' is a perfect translation of 'Deux et deux font quatre'. Now to say that a sentence S is a perfect translation of a sentence S_1, is to say that S and S_1 mean the same. But obviously when the Frenchman uses his sentence he is not making a statement about the English words 'two', 'four', etc. But since the Englishman's sentence *means the same* as the Frenchman's, the Englishman is not making a statement about these words either. Therefore, the necessary proposition 'Two plus two equals four' is not verbal."

All of this is correct but enormously misleading. For it suggests that the French and English sentences are related to the *proposition* in the way that two suits are related to the man who dresses in them. If one's paradigm for a *verbal* statement is one of the following sort: The word "house" occurs in the Bible 10,000 times, More people say "I should go if I could" than "I would go if I could", The word "horse" has five letters —then everyone will agree that necessary propositions are not

[1] Wisdom has shown how it is that metaphysical statements may be true, useless and misleading on the one hand, or false and illuminating on the other. See his "Philosophical Perplexity", *Proc. Aris. Soc.*, 1936-37; and his "Metaph. and Verif." MIND, vol. 47.

[2] Although my discussion is based largely on things I have learned from Wittgenstein and Wisdom, neither of whom have read this, it must not be assumed that they would approve of what I say. (I have read this and I approve.—J. Wisdom.)

verbal, not *about* words, in the way these statements are. This connects with what we have said above—namely, that we *use* necessary propositions in making inferences and in doing calculations. We do not use them as descriptive statements *at all*, and therefore not as descriptive statements about any particular words or phrases.

Suppose that a man is given the job of classifying the people in a certain district according to their professions. He is given only a limited number of headings under which to classify them. One of these headings is "eye doctor", but there is no heading "oculist". He is trying to find to what profession Jones belongs in order to classify him. He sees an advertisement in a newspaper in which Jones announces himself as an oculist, says to himself, "If Jones is an oculist then he's an eye doctor", and puts Jones down under the "eye doctor" heading. According to our ordinary use of "a statement about words", the thing he says to himself would *not* be called a statement about the words "oculist" and "eye doctor". You can say then, if you like, that his statement is *not* verbal. You can say it so long as it does not mislead you—so long as you do not forget that what *makes* his statement *necessary* is the empirical fact that we use the expressions "eye doctor" and "oculist" interchangeably. It is this latter consideration which gives the *point* to saying that necessary statements are really verbal, are really about the expressions which they connect.

But there is an inclination to say that necessary propositions are verbal which comes from a slightly different source. We noticed at the beginning of this paper that some philosophers were more inclined to say "Necessary propositions are not really propositions but are rules or commands" than to say "Necessary propositions are really empirical propositions of a special sort". Now what leads people to say such a queer thing as "Necessary propositions are not really propositions"?

Suppose that a child said to you, "The radio is talking, Daddy". You might reply, "A radio can't talk. You mean that some one is talking on the radio." Or you might reply, "We don't say 'the radio is talking'; we say, 'some one is talking on the radio'." Or you might reply, "Don't say 'the radio is talking'. Say 'some one is talking on the radio'." The last reply could properly be called a *command*. The second reply could properly be said to state a *grammatical rule*. The statement "A radio can't talk" is a *necesary proposition*. That is, you do not mean by it that radios never have talked and never will. It is a necessary proposition closely connected

with the necessary proposition, " A machine can't think ". This example brings out clearly the similarity between necessary propositions, rules of grammar, and commands to use words in a certain way. " A radio can't talk " is a necessary proposition because we have not provided employment for the expression, " The radio is talking ". We could very easily give it employment—namely, in just those circumstances in which we now say, " Some one is talking on the radio ". We teach a child this necessary truth by teaching it that we do not say, " The radio is talking ", by compelling it to use another expression in these circumstances. This brings out the point of saying that necessary propositions are rules of grammar, or commands to use words in a certain way.

Imagine this case : You and a child are watching Siamese twins who are suffering severe pain.[1] Each of the twins locates the pain in exactly the same spot in that region which connects their two bodies. They make cries and grimaces exactly alike, and give exactly similar descriptions of the pain. Afterwards the child says to you, " They had the same pain, didn't they, Daddy ? " You might reply, " We don't say that they had the *same* pain. In this case we say that they had pains *exactly alike*." Or you might reply, " Don't say ' they had the *same* pain. ' Say ' they had pains exactly alike '." By either reply you would be teaching the child the *a priori* truth that " Two persons cannot have the same pain ", or that " you can't have Smith's pain ".

If some one now says, " Is it the case then that necessary propositions are not *really* propositions at all, but are rules or commands ? " our answer again is, Of course not ! According to our actual usage of the word " proposition " it is correct to call $2 + 2 = 4$, Two persons can't have the same pain, valid syllogisms, etc., *propositions*, and incorrect to say that they are *not* propositions. Likewise, if an expert on English grammar were asked to write down as many rules of grammar as he could think of, he would not dream of putting laws of logic, arithmetical propositions, or things like, Nothing can be both round and square at the same time, on his list. Our ordinary use of " rule of grammar " is such that we do not *call* necessary propositions *rules of grammar*. Likewise, as we ordinarily use language we should not dream of saying that $3 \times 3 = 9$, or Nothing can be both red all over and blue all over at the same time, are commands, orders, requests. Thus Ayer was misled if he thought he was

[1] This imitates an example which Wittgenstein has given.

making a *true* statement, as he apparently did think, when he said that necessary propositions are not really propositions, but are rules which can only be followed or disobeyed.

But so long as one is not deceived into thinking that one has made a sort of scientific discovery, has found that necessary propositions are not really propositions after all, there is a great merit in *saying* that necessary propositions are not really propositions. It helps to set forth how utterly misleading it is to say, as has been said, that " the kind of fact to which N (a necessary proposition) corresponds is somewhat different from the kind of fact to which C (a contingent proposition) corresponds, but they correspond in very much the same way ".[1] By emphasising the similarity between necessary statements and rules of grammar or commands to use words in a certain way, it brings out the vast dissimilarity between necessary statements and *descriptive* statements—and makes one want to say that necessary statements really *state* nothing, are not really *statements*, do not state or express facts at all—and makes one see the enormous misleadingness of talking about there being two *kinds* of *fact*, necessary and contingent, in the way in which one may talk about there being two *kinds* of animals, male and female.[2]

[1] D. C. Williams, " The Nature and Variety of the *A Priori* ", *Analysis*, vol. 5, no. 6, p. 88.

[2] I wish to emphasise how much the worthwhile features of this paper are due to Wittgenstein and Wisdom.

V.—KANT'S FIRST ANTINOMY: A LOGICAL ANALYSIS.

By Milton Fried.

Ever since the publication of Kant's *Critique of Pure Reason*, dominant attitudes toward his antinomies have been overwhelmingly sympathetic. Thomists, for example, in keeping with their tradition of cultivating certain irresolvable mysteries which give faith, and consequently theology, a function in the knowing process, welcomed the demonstration that there were problems whose attempted solution by Reason alone, necessarily leads to contradiction. J. Mortimer Adler, the prominent contemporary Thomist, remarks that "To the extent that questions in natural theology concerning God, creation, the soul, are parts of larger questions which require sacred theology, founded on revelation, for their adequate solution, Kant was right in regarding a purely rational approach to the whole question in each case as doomed to the frustrations of an illusory dialectic, a dialectic which did not properly recognise its own limitations in philosophical mysteries ".[1] This, as might be expected, is merely a more general application of remarks made by Aquinas in his treatise on creation, the very problem of Kant's first antinomy. "The world exists", claimed Thomas, "forasmuch as God wills it to exist, since the being of the world depends on the will of God, as on its cause. It is not therefore necessary for the world to be always; and hence it cannot be proved by demonstration."[2] "By faith alone do we hold, and by no demonstration can it be proved, that the world did not always exist. . . ."[3]

Hegel was another who felt that with the antinomies Kant had plumbed the very depths of being to bring to the surface one of the profoundest insights in the history of philosophy. For him they represent the "chief transition to modern philosophy ",[4] and though he is never reluctant to criticise the actual

[1] Adler, Mortimer J., *What Man Has Made of Man*, p. 157.
[2] Aquinas, Thomas, *The Summa Theologica*, Ia pars, Quest. 46, Art. 1.
[3] *Ibid.*, I pars, Quest. 46, Art. 2.
[4] Hegel, G. W. F., *Science of Logic*, vol. i, p. 204.

proofs of thesis and antithesis, or what he considered to be Kant's trivial solution to the problems they raised, he still believed that " to offer the idea that the contradiction introduced into the world of Reason by the Categories of the Understanding is inevitable and essential, was to make one of the most important steps in the progress of Modern Philosophy ".[1] His greatest lament was the fact that Kant had restricted the antinomies to four special problems of Cosmology instead of discerning them in every kind of object, in all conceptions, notions, and Ideas. " Kant ", he complains, " tried to give an appearance of completeness to his four cosmological antinomies by his method of classification, which he borrowed from his scheme of categories. A deeper insight into the antinomous or, rather, into the dialectic nature of Reason shows, however, that *every* concept is a unity of opposite moments, which could therefore be asserted in the shape of an antinomy. Thus, Becoming, Determinate Being, and so on, and other concepts, could each furnish its particular antinomy, and as many antinomies could be set up as concepts were yielded ".[2] " Every actual thing involves a coexistence of opposed elements. Consequently to know, or in other words, to comprehend an object is equivalent to being conscious of it as a concrete unity of opposed determinations."[3] Hegel is thus more than sympathetic to the antinomy. He makes it the central and vital element of his Dialectic, driving thought onward and upward, from concrete universal to concrete universal, until it triumphantly comes to rest in the Absolute idea.

An almost identical devotion to contradiction is shared by Dialectical materialists. In complete sympathy with Hegel's attempt to discountenance modes of thought which supposedly consider objects as isolated, atomic absolutes " detached from the whole vast interconnexion of things ", they took over both his notion of the world as a process, and his key to its development, contradiction. By rejecting his idealism, however, they shifted the *locus* of action from mind to the material universe, and the contradictions detected by Hegel in every notion and conception were consequently transferred to the real objects of the real world.[4]

It is not sufficient for empiricists with antimetaphysical leanings to dismiss these antinomies as examples of the type of

[1] Hegel, G. W. F., *Logic* (Encyclopædia), p. 98 (Wallace Edition).
[2] Hegel, G. W. F., *Science of Logic, op. cit.*, p. 205.
[3] Hegel, G. W. F., *Logic, op. cit.*, p. 100.
[4] Engels, F., *Herr Eugen Duhring's Revolution in Science*, preface and chs. 1 and 13.

undesirable results that follow from metaphysical inquiry. For desirable or not, Kant maintained that they were inevitable transcendental illusions which, instead of vanishing when attention is called to them, must continue to plague the human mind as it swings hopelessly back and forth between the demands of Reason and the demands of Understanding. Understanding, as the faculty of rules, supplies the transcendental machinery (categories and rules) which makes possible any and every unified, intelligible experience. So long as the Understanding is content to restrict its activity to the realm of possible experience it is serving mathematics and physics, and no difficulty develops. When, however it forsakes the realm of possible experience to masquerade as Reason, it must answer to Reason's demands.[1] Reason is the faculty of principles, supplying, not conditions for unifying experience, but Ideas, criteria for organising the concepts of the Understanding, and consequently, experience in general, into an absolutely unified totality. Its fundamental principle is that where the conditioned is given, the entire sum of conditions, including the unconditioned, is also given. This principle, however, is in direct conflict with obligations of the Understanding, which, in dealing with objects of experience, is necessarily limited to the conditioned. The source of the natural antithetic thus emerges. The Understanding, unable to comply with the request of Reason without abandoning the domain for which it is equipped, that of experience, is also unable to reject Reason's demand. Anxious to remedy a situation in which the Idea of Reason is too large for the capacity of the knowing Understanding, and the precise and certain knowledge of the Understanding is too small for the Idea of Reason, the Understanding weakens and surrenders the security of the natural sciences by venturing out beyond the limits of experience to what Kant calls "a dialectical battlefield in which the side permitted to open the attack is invariably victorious, and the side constrained to act on the defensive is always defeated".[2] Here it is possible to prove with equal force two mutually exclusive propositions,[3] *e.g.*, that the world has a beginning in time, that it has no beginning, that there is

[1] Kant seems never to have decided conclusively whether Reason is a separate and distinct faculty of mind or whether it is merely an extension of the faculty of the Understanding. Throughout the critique he shifts from one position to another.

[2] Kant, *Critique of Pure Reason*, p. 394 (Kemp Smith's translation).

[3] This use of the word, "antinomy" was first popularised by Kant, but the conception for which it stands had been used long before by Zeno (the Eleatic), by Plato, and by Aristotle.

freedom, that there is no freedom. One proposition and its proof—the thesis—supposedly embodies the traditional teaching of platonists, while the other—the antithesis—presumably presents the traditional empiricist position. No dogmatic solution to their conflict is possible; for the doctrines involved, far from being deceptions, find the conditions of their necessity in the very nature of reason itself. Since this hypothesis is the only conceivable way of explaining a phenomenon like the antinomies, Kant concludes that the sole possible solution to the problem they raise is his critical one, which would supplant dogmatic metaphysics with his own brand of metaphysics, the transcendental idealism elaborated throughout the *Critique of Pure Reason*.

The remainder of this paper will be devoted to a logical analysis of the proof of the first antinomy in an attempt to demonstrate that neither a new metaphysics nor a new psychology is required to escape from the web of necessary contradiction so neatly woven by Kant. The contradiction that is supposedly proved, we shall maintain, is grounded, not as Kant claims, in any assumption of a world in itself, or in any inborn characteristic of reason, but in an ambiguity of phrase and in a logical procedure which first plants its contradictions and then simulates surprise at discovering them. If our contention is correct, the proofs of the antinomies are nothing more than a philosophical *tour de force*, exhibiting, at their best, a series of linguistic manœuvres.

The thesis of the first antinomy argues for the finitude of the world in two sections. (1) The world has a beginning in time, and (2) is also limited as regards space. Since the space argument is based on the proof of the argument regarding time, we will consider the latter first. Kant's proof is as follows:—

> If we assume that the world has no beginning in time, then up to every given moment an eternity has elapsed and there has passed away in the world an infinite series of successive states of things. Now the infinity of a series consists in the fact that it can never be completed through successive synthesis. It thus follows that it is impossible for an infinite world-series to have passed away, and that a beginning of the world is therefore a necessary condition of the world's existence.[1]

Broken down into its component parts, the above proof is seen to be made up of (1) the contrary hypothesis that the world has no beginning in time, (2) a description of what is involved in this hypothesis—of the quality by which we could determine whether it were true, namely, that an *infinite series* of successive

[1] Kant, *op. cit.*, p. 397.

states of things in the world would have elapsed in time up to every given moment, up to every " now ", (3) a clarification of this quality through a definition of infinite series, and (4) an implicit use of the self-evident fact that there is a " now " in the world. Combining (3) and (4) gives an inference in the *modus tollendo tollens* which yields the contrary of the original contrary hypothesis. From a purely formal point of view the inference is sound, but this hardly makes the proof acceptable.

Before considering what we shall maintain is the proof's major difficulty, it is well to dispose of some of its minor ambiguities. One of them surrounds the precise meaning of the contrary hypothesis that the world has no beginning in time. [Some of its meaning is suggested, of course, by the fact that it involves the passing away in the world of an infinite series of things, but not enough to clear the ground for an adequate discussion of the validity of the proof.] If we look to the antithesis for a clue, we find that a beginning is defined there as " an existence which is preceded by a time in which the thing is not ". Were this what Kant meant in both parts of the antinomy by the word *beginning*, then to say in the thesis that the world has no beginning would be equivalent to saying that it is false that there was a preceding time in which the world was not, or put differently, the world series is coetaneous with the temporal series. But the emphasis in the body of the proof upon infinite series belies this interpretation. In fact, such an interpretation makes the proof incoherent. For what relation is there between the question of whether or not time and the world are coeval, and the elapsing of an infinite series of things ? Being coeval does not necessarily involve being coeternal. Two things can be coeval regardless of whether an infinite series of either has elapsed. But if the word *beginning* does not have an identical meaning in both thesis and antithesis, there is no genuine antinomy. Even if Kant were *able* to prove both that the world has a beginning, and that it has no beginning, he would not have established two contrary propositions, nor even two necessarily related ones, so long as the significant word in both of them was interpreted in two different senses. Strictly speaking, then, either the proof is incoherent, or else this thesis and antithesis are *not* mutually exclusive, and Kant's structure of contradiction collapses.

Since, however, we are still interested in the procedure of each of the proofs, and specifically for the moment in the thesis of the first antinomy, we must come to some decision as to what Kant meant to disprove in the contrary hypothesis that the world

has no beginning. Believers in creationism, whether they refer to the world-series or the temporal series, often talk as if a beginning is a necessary condition of the existence of any part of the series. The beginning on this view is that with which the world or temporal series comes into being in some sense, and without which, never having come to be, it would never exist. Here the definition is loaded with a proof, and if this were what Kant meant, it is hard to see why he found it necessary to introduce another definition, that of an infinite series. To avoid these objections we will take Kant to mean simply that a series has no beginning if every term in it is found to have a predecessor. Such a definition not only relates the problem to infinite series, but does not by itself rule out of the realm of possibility any particular kind of infinite series.

Once this is clear, we can return to the proof of the thesis in an attempt to show that its apparent validity grows out of Kant's special use of the phrase *infinite series*. For him an infinite series is one that "cannot be completed through successive synthesis", or put in another way, "the concept of infinitude is this, that the successive synthesis of units required for the enumeration of a quantum can never be completed".[1] And the more Kant talks of it the more subjective and mental the series seems to become. It is as if its infinitude were an infirmity of mind, the mind struggling in vain to complete its assigned journey by stepping from one member to the next, by counting itself out. Stripped of these mental overtones, his definition is quite close to the etymological meaning of the word *infinite*, but like so many other etymological meanings, it nurtures ambiguities and psychological associations which warrant discarding it for all but literary purposes. Mathematicians have long seen this, and for over a quarter of a century have operated with a not only non-contradictory, but non-ambiguous concept of infinity. That many philosophers have not yet followed suit speaks eloquently of cultural lag.

The proof's major ambiguity lies in the meaning of "completed through successive synthesis". If Kant thought of this loosely, imagining that a series is not infinite if the mind, catching a glimpse of the end of the series, can somehow complete a journey to it, then it is easy to conceive of the kind of speculation which might have led him to accept his proof. For on this view, what may ultimately determine whether a given series is infinite, is not so much any particular property of the ordered sequence itself, but rather, the direction you take as

[1] Kant, *op. cit.*, p. 401.

you proceed in imagination from one term to another. To illustrate this, we will use a regression of the type "$*w$", specifically the series of negative integers including zero, arranged in the usual order, thus:

$$\ldots, -3, -2, -1, 0.$$[1]

Examining it from right to left we find that every term has a successor, and that it is consequently impossible to complete the series through successive enumeration. By Kant's definition, therefore, the series is infinite. In making the return journey, however, a difficulty is encountered: Where begin in a series which by definition has no first term? The tendency is to start at the very point at which the mind stopped when it travelled from right to left. This creates the illusion of being able to complete the successive synthesis at zero, and the previously infinite series seems to become finite in virtue of the change in direction. The same thing happens in Kant's series. He considers the present, the "now", as the end of the temporal series. This, plus the initial assumption of his proof that the world has no beginning in time, gives him a series similar to the one discussed above—open at the left and closed at the right. Moving from right to left, from present to past, this series (whose beginning the proof has removed by postulation) goes on without end. From this point of view it is infinite, and we are able to say with Kant that "there has passed away in the world an infinite series of successive states of things". If, however, we retrace our steps, covering the series from left to right, we are brought to an abrupt halt by the present. Now the same series seems to have an attainable end. No longer infinite by his definition, it seems natural for it to have elapsed. This brings Kant face to face with the specious problem which is the heart of his proof, and which he summarised in his observation on the first antinomy as "the difficulty involved in a series that is infinite and yet has elapsed".[2] If he did not make completely clear to himself that this problem was only apparent, being caused by a change in direction which was only possible because the mind chose an arbitrary beginning for its return journey, he would certainly have sought some explanation. A possible one would have been that the very assumption of a world with no

[1] Mathematicians customarily divide series into four groups distinguished by the presence or absence of extreme elements. Thus the series of the type "w" is a discrete series which has a first element but no last. Type "$*w$" is a discrete series which has a last element but no first, etc. See E. V. Huntington, *The Continuum*.

[2] Kant, *op. cit.*, p. 401.

beginning led to the strange state of affairs, and that it must therefore be possible to disprove it.

It must be remembered that the foregoing is not meant as an attack on the proof, but as an imaginative reconstruction of considerations which might have occurred to Kant in view of his ambiguous conception of an infinite series, and which might have suggested the proof. As an attack it would be unsatisfactory inasmuch as the wording of the proof nowhere makes explicit reference either to the direction in which the mind travels as it makes its "successive synthesis", or to arbitrarily starting what we have called the return journey at some particular point in the series.

Not only is it unnecessary to make the above assumptions, but it is possible to give greater definiteness to the Kantian notion of "completed through successive synthesis" and still criticise the proof. Suppose Kant meant by "successive synthesis" something like reciting in order[1] the names of the numbers, *e.g.* "zero", "minus one", "minus two", "minus three", etc. Assume too that a synthesis has to be made separately for each member of the series regardless of the order in which the members are spoken. [In other words, there can be no arbitrary starting-point when proceeding from left to right.] Suppose also that by "completing the successive synthesis" he meant calling the roll of the entire series. His definition of an infinite series as one which "can never be completed through successive synthesis" would still be ambiguous; for on his view, whether or not any particular series is infinite ultimately depends on information not even mentioned in the definition, namely, a time specification. By changing this specification the same series can be made either infinite or finite. Any discrete series, for example, can be "completed" in an unlimited amount of time, *i.e.*, given any series, no member of it can be mentioned which will not be reached and passed if we are allowed an infinite time. Better still, take the regression we employed before. If one could read faster and faster, that is, one could say "zero" in a second, "minus one" in half a second, "minus two" in quarter of a second, "minus three" in one-eighth of a second, and so on, then all the names could be read in just two seconds. But if you specified that the same amount of time were to be taken to read each number, and also

[1] We said "something like reciting in order" because it need not be exactly that. It might, for example, refer to the formation of mental images of each of the numbers, in fact anything that takes time and involves an order of succession.

that the reading was to be stopped at some particular time, then the series could never be "completed". By Kant's definition the same series would be finite in the first case, and infinite in the second. From this it should be clear that any series which Kant might call infinite would be infinite [*i.e.*, would actually fit his definition] only if three conditions—nowhere stated in the proof—were fulfilled. In the first place, one must start making the "synthesis" at some particular time. Otherwise it will be possible to claim an infinite time for the "synthesis", and as we have indicated above, any series can be completed in an infinite amount of time. Second, there should exist a temporal unit t such that it requires at least t seconds for each "synthesis". Finally, one must stop making "syntheses" at some particular time. If any one of these three conditions is lacking, every series can "be completed through successive synthesis". Now, since in Kant's series of states of the world neither the first nor the second of the above conditions is satisfied, he is not justified in claiming that "if we assume that the world has no beginning in time, then up to every given moment . . . there has passed away in the world an infinite series of successive states of things". This series would be infinite only if Kant were able to show that the temporal series was finite. But he never bothered with the temporal aspect. In fact, it is unlikely that he was even aware of its relation to the problem. Hence, his conclusion "that it is impossible for an infinite series to have passed away" does not follow. How misleading the language of the proof may become can be glimpsed from the above quotations. When it is said that "there has passed away in the world an infinite series", or "that it is impossible for an infinite series to have passed away", one feels as if every term of this endless series has paraded before one, and that the entire membership is now somewhere beyond the reviewing stand. The immediate response is that it is indeed impossible. How could an infinite number have passed and gone? But it is the imagination distorting. Actually the review continues. States of the world still pass. And those that are gone could have taken an infinite time to get out of sight.

It thus becomes apparent that whatever plausibility Kant's argument possesses it derives, in general, from linguistic sources, particularly from the ambiguity in the phrase, "completed through successive synthesis", and the consequent inadequacy of his definition of an infinite series. Overlooking this, he accepted as genuine the sophistical problem supposedly involved in an infinite series elapsing, and proceeded to eliminate it by

concluding that the assumption with which the proof started—the world has no beginning—is impossible. What in the light of modern mathematics this absurdly maintains [by analogy to the numerical series we employed as an example] is that an infinite series open at the left and closed at the right, a series of the type " $*w$ ", is impossible.

From the foregoing it should not be construed that Kant was aware of the ambiguity in his use of the word infinite, that he deliberately took advantage of it in the interest of the logical architectonic of the *Critique*. Nothing could be further from the truth. For one thing, he was interested in a correct use of the concept of infinity as early as 1770. For another, he was convinced that his proofs arose naturally out of the matter in dispute. In his observation on the first antinomy he explicitly asserted his determination not to establish his thesis in what he contemptuously considered "the usual manner of the dogmatists, by starting from a defective concept of the infinitude of a given magnitude".[1] In addition, a long footnote of his *Inaugural Dissertation* is devoted to those of his contemporaries who operated with contradictory notions of the infinite. It is worth quoting if only because it summarises an attitude toward the problem of infinity that Kant never changed.

> Those who reject the actual mathematical infinite spend no great pains over it. The definition of the infinite which they put together is such that out of it they are able to carve some contradiction. The *infinite* is for them *a quantum than which a greater is impossible:* and the mathematical infinite is an aggregate (of some assignable unit) than which a greater is impossible. But, as they here define not "infinite" but "maximum", and a greatest aggregate is impossible, they easily infer the impossibility of this infinite which they themselves have invented. Or alternatively, they call an infinite aggregate an infinite *number*, and show that this is absurd, and so it clearly is. But again they are fighting only with figments of the mind. If they had conceived the mathematical infinite as a quantum which, in relation to any unit of measurement, is *an aggregate greater than any number;* if, further, they had remarked that measurability here implies reference to the sort of comparison used by the human intellect, one in which any definite concept of an aggregate can be attained only by successive additions of unit to unit, and the complete conception of it which is called number only by completing the summation in a finite time; they would then have clearly seen that things which are at variance with a certain law of a particular kind of [knowing] subject do not thereby pass beyond the sphere of all intellection, since an intellect is possible which might apprehend an aggregate distinctly at a glance, without the successive application of a measure, though such an intellect would not indeed be human.[2]

[1] Kant, *op. cit.*, p. 399.

[2] Kant, *Inaugural Dissertation and Early Writings on Space*, p. 37.

Considerations similar to those already discussed hold for the proof of the second part of the thesis, that the world is limited in regard to space. Here, instead of dealing with the simple temporal series, the emphasis is on a synthesis completed through the continuous addition of spatial units in time. The gist of the proof is that "an infinite aggregate of actual things cannot be viewed as a given whole" [1] because—

> in order to think, as a whole, the world which fills all spaces, the successive synthesis of the parts of an infinite world must be viewed as completed, that is, *an infinite time must be viewed as having elapsed* in the enumeration of all coexisting things.[1]

Here again, in his use of phrases like "in order to think" and "time must be viewed as", Kant betrays his subjective psychological interest, and provides the grounds for Kemp Smith's criticism of his proof as committing the fallacy of *ignoratio elenchi*, as invalidly arguing from *the subjective impossibility of apprehension* to the *objective impossibility of existence*. But of greater significance to us is the dependence of this part of the thesis upon factors in the proof of the first part. This can be gleaned from the italicised sentence, and from the constant repetition of the notion that an infinite magnitude "can be thought *only* through a synthesis of its parts . . . *only* through a synthesis that is brought to completion through repeated addition of unit to unit".[2] Accepting the latter misapprehension is impossible in the light of George Cantor's revolutionary work on the theory of infinity. There is no need to present his detailed development of the concept. A vast literature of the subject matter is available. It is sufficient to indicate that following his lead the procedure of modern mathematics treats infinity as basically a property of classes, and derivatively a property of series [which are ordered classes]. An infinite class, on this view, is a class which can be put into one-to-one correspondence with a part of itself.

At least two things of importance for us emerge here: one, that it is possible to construct a completely unambiguous concept of the infinite which is sufficient to handle all the practices and problems traditionally associated with and obscured by older notions of infinity; two, that in spite of the fact that an infinite class cannot be completed by enumeration, it can be given all at once, as a "totality", by giving the common, defining property peculiar to its members. In practice this second procedure is very much like our method of dealing with large finite classes.

[1] Kant, *Critique*, *op. cit.*, pp. 397-398. The italics are mine.
[2] *Ibid.*, pp. 397-398.

For when we make reference, say, to the class of American citizens, we do not go through the tedious and time-consuming process of enumerating each individual member. We usually identify and operate with the class by indicating the properties defining membership in it. Taking the above facts into consideration, there is no longer any reason for enveloping the concept of infinity in the penumbral haze of psychological associations which make a travesty of knowledge by glorifying an unintelligible paradox.

Like the thesis, the antithesis has two parts: (1) The world has no beginning in time, and (2) is unlimited as regards space. First, the proof of the former.

> . . . let us assume that it has a beginning. Since the beginning is an existence which is preceded by a time in which the thing is not, there must have been a preceding time in which the world was not, *i.e.*, an empty time. Now no coming to be of a thing is possible in an empty time, because no part of such a time possesses, as compared with any other, a distinguishing condition of existence rather than of non-existence; and this applies whether the thing is supposed to arise of itself or through some other cause. In the world many series of things can, indeed, begin; but the world itself cannot have a beginning, and is therefore infinite in respect of past time.[1]

One might begin a discussion of the proof by quarrelling with Kant's notion of a beginning as " an existence which is preceded by a time in which the thing is not ",[2] insisting that there were no absolute beginnings, that in actual practice the word beginning was used as a signpost to indicate that, for the purpose at hand, a particular event was to be considered the point of departure. But this would neither meet Kant squarely on his own ground, nor be in keeping with the original purpose of the paper. Accepting Kant's formulation, then, the proof of the infinity of the world-series depends upon the fact that on the contrary assumption an empty time must constitute the limit of the world, and beginnings are impossible in empty time. One possible objection —and one of which Kemp Smith makes much—is the ambiguity in the word " world " which he suggests may refer either to the " material world ", or to " all that is ".[3] Be this as it may, so much is clear from Kant's observations on the antithesis: by " world " he does not mean some " intelligible world ", for " the *mundus intelligibilis* is nothing but the general concept of a world in general, in which abstraction is made from all the conditions of its intuition, and in reference to which, therefore, no synthetic proposition, either affirmative or negative, can possibly be

[1] Kant, *Critique*, *op. cit.*, p. 397. [2] *Ibid.*, p. 397.
[3] Kemp Smith, *Commentary to Kant's ' Critique of Pure Reason '*, p. 487.

asserted ".[1] What he does mean is the sensible world of appearances, his *mundus phenomenon*.

If, with this in mind, we paraphrase the first two sentences of the proof, we get the assertion that to assume the sensible world has a beginning means that the sensible world was preceded by a time in which this sensible world was not. Ordinarily, such a statement would presuppose that at least some segment of time was not part of the sensible world, was in some sense independent of it. Otherwise, the notion of a "preceding time" would be nonsensical. It consequently hardly follows that a "preceding time in which the world was not" is an empty time. At most, it is a time unoccupied by the *sensible* world. It need not, as he suggests, be empty of *all* content. Metaphysicians might maintain that it had temporal content. And from a logical point of view it might very well be a storehouse for other contents as non-sensible as time itself, contents which may originally have functioned as a creative source of the world.

There were times when Kant might have realised this,[2] but his proof appeared natural and valid because what he had in mind was not so much the emptiness of the time before the world as the impossibility of such a time. That the world had a beginning meant to him, we saw, that a time preceded the world in which the world was not. This was the contrary assumption he was forced to make by the formal structure of his proofs. But it was one he was not prepared to grant, and psychologically probably never did. Witness this quotation from his observations on the antithesis:

> I am aware that attempts have been made to evade this conclusion [that the world is infinite as regards both time and space] by arguing that a limit of the world in time and space is quite possible without our having to make the impossible assumption of an absolute time prior to the beginning of the world, or of an absolute space extending beyond the real world.[3]

In other words, it is suggested that by making the impossible assumption of an absolute time prior to the beginning of the world, it *is possible* to argue for a limit of the world. Yet, it is precisely this impossible assumption that he employs to demonstrate the *impossibility* of arguing for a limit of the world. What he is saying is that the same assumption from which you infer the finitude of the world enables you to deduce its infinitude. But this is beside the point. The significant thing for us to note

[1] Kant, *Critique*, *op. cit.*, pp. 401-402.

[2] In the context of some of his extreme realist passages, *e.g.*, the latter part of the quotation on page 215 of this paper.

[3] Kant, *Critique*, *op. cit.*, p. 399.

is what we already called attention to in our discussion of the proof, that his conclusion (the infinity of the world) is not a logical consequence of his assumption (of a time preceding the world in which the world is not). It does follow from his conviction that the assumption is impossible, from his insistence that there can be no absolute time prior to the world. Now this attitude toward time is neither explicitly mentioned nor defended as part of the proof. It is simply taken for granted, and allowed to function as a tacit assumption. But it accomplishes no less on this account. Not only does it create an illusion of validity by begging the question of the proof, but it smuggles into the body of the proof the transcendental principles of Kant's *Æsthetic*. Evidence of this latter procedure abounds in the text. In a footnote to the observations on the first antinomy he remarks that " . . . an empty space *within the world*, is at least not contradictory of transcendental principles and may therefore, so far as they are concerned, be admitted ",[1] which by indirection suggests that any empty space outside the world does contradict his transcendental principles, and may not, therefore, be admitted. Then again, there is the proof of the second part of this antithesis, which is little more than a reaffirmation of Kant's views on the transcendental ideality of space. As regards the infinity of space, he says :

> . . . let us start by assuming the opposite, namely, that the world in space is finite and limited, and consequently exists in an empty space which is unlimited. Things will therefore not only be related *in space* but also related *to space*. Now since the world is an absolute whole beyond which there is no object of intuition, and therefore no correlate with which the world stands in relation, the relation of the world to empty space would be a relation of it to no *object*. But such a relation, and consequently the limitation of the world by empty space, is nothing. The world cannot, therefore, be limited in space ; that is, it is infinite in respect to extension.[2]

The effectiveness of this proof obviously depends upon the force of the sentence which claims that since the world is an absolute whole beyond which there is no object of intuition, there is therefore no correlate beyond it with which the world stands in relation. Space, in other words, to be the limit with which the world stands in relation—in fact, to be anything at all, must be at least an object of intuition. But if it were an object of intuition, it would be part of the absolute whole which is the sensible world, and by definition there could be nothing sensible beyond it to limit it. This is the transcendental concept of space

[1] Kant, *Critique*, *op. cit.*, p. 400. [2] *Ibid.*, pp. 397-398.

as merely the form of outer intuition,[1] and it is this concept which is the big stick of the proof, bullying it to its conclusion. It boldly asserts without proof what is supposed to be proved, and makes an empty pretence of the form of proof which presumably proceeds by assuming the contrary of the position to be proved.

The technique can be crystallised into a simple formula. First, you grant the opposite conclusion (to be reduced to absurdity) that the world is finite—existing in an empty space which is unlimited. Then you realise that in accordance with your own ideas on space the notion of an unlimited empty space is impossible. Hence, you withdraw from the contrary conclusion the generous reprieve previously granted, and conclude, from the mere juxtaposition of the two propositions, that your own has been proved necessarily correct.

The dependence of the proof upon the transcendental principles of the *Æsthetic* testifies to a fatal flaw in the entire antinomial structure. For one thing, their use contravenes Kant's avowed intention of employing the dogmatic proofs of traditional philosophy both to emphasise the "naturalness" of the antinomy, and to demonstrate how his critical teaching avoids historic pitfalls. Notice that in one of the key sections of the *Critique*, the one entitled, "The Interest of Reason in these Conflicts",[2] Kant anchors his antinomies in the past, in the traditional, dramatic conflict between the speculative and practical interests of Reason, which, for him, "constitutes the opposition between Epicureanism and Platonism". For another, most commentators are agreed that in the intellectual biography of Kant preoccupation with the problem of the antinomy antedates the views on space and time expressed in the *Inaugural Dissertation* and the *Critique*. In fact, says Kemp Smith, "It was the existence of antinomy which led Kant to recognise the subjectivity of space and time".[3] But since, as we have seen, this attitude toward space and time has to be assumed to prove one side of the antinomy, Kant is placed in the anomalous position of having adopted a notion (the ideality of space and time) in order to explain a situation (the antinomy) which can be shown to exist only by tacitly assuming this notion (the ideality of space and time). He is in the hopeless predicament of the man who pursues his shadow to keep it from getting ahead of him. He is chasing himself.

[1] Of this there can be no doubt, for Kant attempts to reinforce this proof by appending a short summary of the teachings of the *Æsthetic*. See Kant, *Critique*, *op. cit.*, p. 398, note b.

[2] Kant, *Critique*, *op. cit.*, p. 422.

[3] Kemp Smith, *op. cit.*, p. 432.

VI—DISCUSSIONS.

THE RIGHT AND THE GOOD.

MAY I offer some reply to the strictures so courteously passed by my friend Prof. de Burgh, in MIND, N.S. 192, on my essay with this title? I doubt whether I shall satisfy him, or even myself, *ἀλλ' ὅμως εἰρήσεται*. There are some parts of his remarks which I do not feel called upon to counter, since they do not appear to impugn anything I have written. This is particularly true of all that is said at MIND, N.S. 192, pp. 495-496. The quotation there made from an unreported speech by me about a supposed "Japanese airman" seems to me to have nothing to do with the question about the relation of *good* to *right*. What I meant to ask, if I really must "say it in words of one syllable", was whether an airman, who had been exceptionally diligent in bombing women and children, could conceivably be welcomed *for his painstaking efforts in that task* at the "great Assize" with the words, "inasmuch as ye have done it unto the least of these my brethren . . . ," and that question, I think, answers itself. This is not the only case in which Prof. de Burgh seems to have misconceived my meaning, perhaps by some fault of my own. On p. 495 he quotes me as having elsewhere (*Philosophy*, vol. xiv, no. 55) *accepted* the maxim of Trimalchio in Petronius, *nec turpe est quod dominus iubet*. The words were, in fact, adduced simply to show what consequences must follow from the denial that the same man is *dominus sui*. Since I was accepting the doctrine of the schoolmen that the same man *is* his own *dominus*, I meant, of course, to be repudiating these conclusions, and I do not think that Prof. de Burgh could have misunderstood me thus if he had remembered the application Trimalchio makes of his maxim.[1]

Nor, again, in the sentence quoted from my essay at the bottom of p. 495 was I troubling myself with the problem which Prof. de Burgh pronounces unmeaning. I was thinking of a real problem, that raised by Mill's well-known assertion that "the motive, when it makes no difference in the intention makes none in the morality of the action," and intending to suggest against Mill that the supposed case never arises; a difference in "motive" *always* makes a difference in "intention." Since Prof. de Burgh himself requires of a right act that it should be done as a discharge of *duty*—and not, for example, with an eye to reward or "publicity", I imagine he agrees with me here.

Once more, I agree with Prof. de Burgh about the meaning of the famous sentence with which Kant opens his *Grundlegung*. But I

[1] He is, in fact, avowing that he had been simultaneously the *παιδικὰ* of a lustful master and the play-boy of an equally lustful mistress, and taking credit to himself for both performances.

do not see that it is a "monstrous paradox" to say that a *morally* good will is the only thing which is always and unconditionally "good in any sense" (which is what I take Kant to be saying). It is *arguable* that this thesis is mistaken, but equally arguable, as I think, that it is correct. What would be "monstrous" would be the statement that nothing but the good will ever or anywhere has any value at all, and *this* statement Kant never made. And I still think that my account of the argument of the *Grundlegung*—I do not say of that of the *K.d.pr.V.*—is correct. Kant's "syllogism" here is (1) the only thing which is always good is the morally good will; (2) the morally good will is the will always to do right simply because it is right; (3) *ergo*, the will to do right because it is right is the only thing which is always good. No doubt the relation here asserted between *right* and *good* is, in Kantian terminology, synthetic, not analytic, but it is still inconsistent with any "radical duality of the moral standard".

To come to the parts of Prof. de Burgh's discussion which do directly impugn my own position, I seem to find in his pages [1] three different, and, as I think, incompatible theses. (1) *In ethics* itself there is a fundamental and irreducible "duality of standard". This seems to mean that there are two distinct and irreducible types of *moral* action, obedience to a law of duty because "it is the law", and action "for the sake of the good". (2) "Action for the sake of the good" is not really *moral* action at all; it belongs to the religious, as distinct from the ethical, life (and religion, according to Prof. de Burgh's Gifford lectures, is essentially not *praxis* but *theoria*). (3) "There is a higher form of *praxis* than the ethical, and a higher motive of action than even reverence for the moral law." Statements (1) and (2) seem to me formally incompatible, and (1) and (3) only compatible on the condition that we accept without any reservations the view that the religious life is one of *theoria* pure and simple. And I find more than one difficulty here. If all action *sub specie boni* is specifically religious, not ethical, I conceive that we shall have to say of a man like Bentham [2] that he was *consciously* a profoundly religious man, and this I do find a "monstrous paradox". If the "religious" life is one of *praxis* at all, that is, if the "higher motive" is a genuine *motive* to "choice or avoidance", how can its existence be simply ignored in ethics, which is either a doctrine of the principles of "choice and avoidance" or is nothing at all? *Per contra*, if the "higher form of *praxis*" and its motive are, as Prof. de Burgh calls them, "supra-moral", how does their existence prove a "dualism of *ethical* first principles"? Unless action *sub specie boni* is *moral* action, the whole case for a radical

[1] I mean in the pages of his contribution to MIND; but I seem also to find the same three theses, insufficiently distinguished, running through the argument of *From Morality to Religion*.

[2] I am thinking, of course, of Bentham's known and violent animosity against the very use of the word *ought* in moral and political reasoning.

dualism within ethics falls to the ground; if it is moral action, there can be no coherent thinking about moral problems unless there is some intelligible connexion between the "standard" of *right* and that of *good*, any more than there could be coherent thinking in physics if the physicist had to work with two standards of mass, gravitational and electro-magnetic, and could establish no kind of relation between them.[1]

All this, it may be said, only goes to show that one of the proposed standards must be ultimate, not that this ultimate standard must be that of *good* rather than that of *right*. But Prof. de Burgh has surely cut himself off from making that rejoinder when he speaks of action *sub specie boni* as the "higher form of *praxis*". To be consistent with himself, *he* should say that the very notion of action inspired by an *ideal* of good, however "secular" your ideal may be, is "supra-moral" and belongs to "religion". *Ethics* should be able to dispense altogether with the very notion of an "ideal", it should be a mere matter of "obedience to orders". But can it ever be that and nothing more? A study of Hobbes should be instructive on the point. No one has ever tried harder than Hobbes to construct an ethics without an ideal on the single basis of the notion of duty. It is true that, according to him, all obligation to virtuous action is based in the end on the duty of obeying the dictates of the "natural law", and that the "natural law" is also the commandment of God. But the knowledge that it is a commandment of God is supposed to be possessed only by men who have received the Scriptures and learned *from them* that God requires obedience to the natural law. All that the rest of mankind, who have no "revelation", can know about God is, according to Hobbes, that God is the immensely powerful "cause of the universe"; that He is good they have no means of knowing. Hence, even the consideration that their own reason, like everything else in the universe, comes from God, and that this reason enjoins obedience to the natural law, affords no proof that the "commandment is good"; it is the commandment, and that is all that there is in principle to say about it. An ethics built up on such a basis ought to be, if any ethics can be, one without any ideal. Yet Hobbes never really gets away from the conviction that God really is good and that there is an ideal. This comes out quite curiously, for example, in the vehement polemic of the *Leviathan* against the traditional doctrine of everlasting torments after death. Hobbes is determined to explain away any passages of the Scriptures which seem to countenance this doctrine, and his avowed reason is that it would be inconsistent with the goodness of God, though, according to his own metaphysics,

[1] Prof. de Burgh may reply that he has formally proposed to distinguish between the *ethical* and the *moral*. But my difficulty is about accepting this proposal. After all *moralis* is only Latin for ἠθικός, as *equus* is Latin for ἵππος. A principle of "choice and avoidance", it seems to me, *must* be one for the regulation of our *mores*.

we have no warrant for ascribing to God goodness, or any other positive attribute, and therefore no warrant for holding that an interpretation of Scripture which would imply that God is not good must be a misinterpretation. (Incidentally, his earnestness in this matter completely destroys the popular conception of him as a free-thinking scoffer. If he had been that, why should it have mattered to him whether God as depicted in Scripture is good or not ?)

Against what seems to be one at least of Prof. de Burgh's views, I hold, then, that the conception of an ideal, which must bring with it the notion of *good* and action *sub specie boni*, is no importation into ethics from a " supra-moral " sphere, but is vital to ethics itself. And I would add that when once you begin to talk of religion as " supra-moral ", your religion is in as grave danger of degenerating into an amoral or even anti-moral *praxis* as your morality is of becoming nothing more than subservience to the decrees of a dictator with his Ogpu or Gestapo at his back.

The point really at issue between Prof. de Burgh and myself is, I should say, how exactly morality differs from religion. I am not at all sure that I have any satisfactory answer to give to that question, but I submit that Prof. de Burgh's way of answering it is altogether too summary, since I am at least sure (1) that the required distinction cannot be made by assigning the notion of *right* to ethics and that of *good* to religion, and (2) that we shall only falsify our conceptions of religion if we allow ourselves to introduce into them the Aristotelian hard-and-fast antithesis of *theoria* and *praxis*. I cannot conceive that either Plato or St. Paul would ever have followed Aristotle on this point without grave reservations, and I may remind Prof. de Burgh that St. Thomas, with all his reverence for Aristotle, felt bound to assert in explicit contradiction of him that the " practical virtues " are to be found in God. I gravely doubt whether any absolutely hard-and-fast line of demarcation can be drawn at all between the moral and the religious life. I think I should say that a *praxis* which remained definitely and altogether non-religious would in any case be something inferior as morality,[1] and, on the other hand, that a religious life which is really active is never merely *theoria*, though it is, no doubt, a *praxis* which draws its inspiration from *theoria*. But, for the matter of that, is not the Kantian ethic, for example, also in its way a *praxis* inspired by *theoria*, by the " contemplation " of a genuinely free will " raised above all the causality of the sensible world " ?

With respect to the particular suggestion that morality can work simply with the notion of *right*, leaving that of *good* on one side as belonging properly to religion, I have something more to say. I do

[1] This is not really inconsistent with my foregoing remark about Bentham. For I should say that Bentham's doctrine is not merely a non-religious ethic, it is also, from the strictly ethical point of view, an inferior type of ethic, and it is so precisely for lack of an adequate ideal of good.

not suppose that Prof. de Burgh means to revive the Stoic paradox that all "transgressions" are equal, since they are all alike departures from the right. He will probably agree that an act of wanton cruelty, for example, is a graver "transgression" than one of unpunctuality, though neither is right. Now by what standard does morality—a morality as untouched by religion as you please—estimate the relative gravity of "transgressions" of the law? All transgressions are wrong, just as all lines which are not straight are curved. But one curved line may be more curved than another, though neither is straight. A circle of large radius is, as we know, less curved than one of smaller radius. We can make the statement intelligibly because we have in the notion of the "radius of curvature" a standard of estimation. What is our standard when we compare the relative gravity of departures from right? I do not see where to find it unless in the *badness* of the acts compared. If this is so, either we must accept the Stoic paradox of the equality of all transgressions or we must admit *good* and *evil* by the side of *right* and *wrong* as notions which belong properly to the sphere of ethics. We cannot escape or mitigate the alleged irreducible duality of the moral standard, as Prof. de Burgh half seems inclined to do, by dismissing the notion of *good* from ethics to religion. But an ethics with two radically discrepant standards seems to me to be no rational discipline at all. If our ordinary moral judgments seem to betray such a duplicity of standards, that, I should say, is proof that in our more unreflecting moments we have not got to the bottom of the matter. The apparent duality of standard only indicates that we are confronted with a problem which demands solution but has not yet been solved.

The case would be altered if it were possible to maintain either (1) that there are right actions which are in no sense good, so that it would be possible for a man to spend his life in unremitting doing of right without ever doing a good action, or (2) that there are good acts which are not right, so that a life-time of good deeds might be taken up with doing what is wrong. But I do not believe that anyone seriously defends such a notion.[1] To talk, for example, of such things as the punctual and accurate payment of one's household bills as right but not good seems to me to amount to showing yourself blind to the real goodness of the discharge of prosaic daily duty, and not far from the error of the persons so much dreaded by Kant, who waste their lives waiting for opportunities of melodramatic "heroism" which never come. And as for good actions which are not right, I can see no meaning to the phrase unless it means acts which might be right in a different agent and with a different situation, but as the acts of this agent in this concrete situation are no more good—though they may, no doubt, be evidence of "good dispositions"—than they are right.

A. E. Taylor.

[1] Prof. de Burgh certainly does not, as his Gifford Lectures make abundantly plain.

TWO FUNCTIONS OF THE INTELLECT.

THE dispute between empiricists and rationalists in philosophy has always revolved round the question of the cognitive capacities of the human intellect. Those who favoured empiricism have denied that the mind is the source of any positive knowledge: all truths, they have said, ultimately depend upon the data of sense-experience and introspection. Against this rationalists have claimed that there are certain very important propositions which can be apprehended by thought and thought alone. The difference is obviously one of crucial importance. In this note I do not propose to try to say anything decisive on either side's behalf, but merely to point out that the question at issue can be made more precise, and certain errors which have been committed in the past avoided, if we take account of a distinction which, so far as I know, is made most clearly in Kant's inaugural *Dissertation* of 1770.[1]

In that work Kant distinguishes (§ 5) between two functions which the intellect may be said to perform. He calls them "uses" of the intellect. "By the first use, the very concepts of objects or of relations are *given*, and this is the *real use*; by the second use, concepts, whencesoever given, are only *subordinated* to one another, the lower to the higher (the common marks), and compared with one another according to the principle of contradiction; and this is called the *logical use*. The logical use of the intellect is common to all sciences, but not the real use. For a cognition, however given, is regarded as contained under a mark common to many; and this either immediately (as in judgments, which aim at distinct knowledge) or mediately (as in reasonings, which aim at adequate knowledge). Thus, sensitive cognitions being given, the cognitions are subordinated by the logical use of the intellect to other sensitive cognitions as to common concepts, and as phenomena to more general laws of phenomena." But this intellectualising of sense presentations, as Kant is careful to point out, does not mean that they become intellectual *in sensu reali*: does not mean, that is to say, that we can say that the intellect is the source of our knowing them. On the contrary: however much we intellectualise sensitive knowledge, it still remains

[1] The distinction remains in some isolated passages in the *Critique of Pure Reason*: see, *e.g.*, B 92 = A 67, B 359 = A 303, B 676 = A 648. The term "real" is, however, mainly replaced by "transcendental" in the *Critique*, and the distinction generally gives way to the analogous (and fundamentally identical) one between synthetic and analytic *a priori* conceptual knowledge. In quoting from the *Dissertation*, I make use of the translation by J. Handyside.

fundamentally sensitive. Truly intellectual knowledge, which is the product of the intellect in its real use, is quite different from this. It consist of a body of propositions based on concepts which are not derived in any way from sense-experience but are rather the product of the intellect itself. In its real use the intellect does not merely arrange data presented to it from another source but itself produces data; and that is why, of the two "uses", the real use is much the more important.

I do not propose to discuss here the details of Kant's view in the *Dissertation*, but rather to try to bring out its philosophical importance and relation to the controversy between rationalism and empiricism. The main point I have to make is that both rationalists and empiricists must admit that the intellect has a function corresponding to that which Kant describes under the "logical use", but that this has ultimately no bearing on the solution of the dispute between them. For that turns on whether or not the intellect has also a "real use". At the same time, we shall see that neglect of the Kantian distinction has led to some false arguments being advanced by rationalists and some absurd denials made by empiricists.

What, in ordinary language, does Kant mean when he calls attention to the logical use of the intellect? I suggest that he is referring to certain very obvious intellectual activities. First of all, he is referring to the formation of universals by processes of abstraction. For organised knowledge, or, for that matter, for language itself to be possible, we need universals; but we cannot see universals or apprehend them by any sense activity. To form universals an activity of the intellect is required; and it is an activity of the intellect in its logical capacity. Secondly, if we are to make any use of isolated pieces of knowledge, the logical relations of different universals must be traced, and "lower" concepts be subordinated to "higher": this too is "work for the mind". And again, science, and everyday discourse too, need general propositions, and the subordinating of less to more general propositions; but this again is a task which the senses cannot perform for themselves. It is the intellect which forms general propositions and traces their relations; the intellect, too, which deduces from any given propositions anything which can be inferred from them by use of the laws of logic. In all this the intellect is concerned in what may be called its logical capacity. And we may notice immediately that it is a strictly subordinate capacity: it cannot be exercised unless sense-data, of one kind or another, are forthcoming for it to work on. It is a capacity which, as Kant says, is operative "in all sciences" and in all departments of human thought; but it is not any the more originative because of that. In its logical use the intellect is, quite strictly, the servant of the senses.

Now the question I wish to ask is whether anyone, whatever his views on the controversy between rationalism and empiricism, can deny that the intellect does perform this logical function. Surely no

one can, at least with any plausibility. For quite plainly the activities I have described are performed, and quite plainly they are not performed by the senses. If they are not to be ascribed to the intellect, to what should we ascribe them? Yet I think there have been some empiricist philosophers who have been inclined to deny that the intellect has even a logical use; I am not sure, for instance, that Hume can be acquitted of the charge. In saying that the contents of the mind are impressions and ideas, and that an idea is a less vivid impression, Hume seems to be trying to deny that there is any difference in kind between a universal and a sense-particular, and so to be denying the presence of an intellectual activity. Yet surely this (if the facts are truly described) is a gross mistake.

Why is it that empiricists have been reluctant to admit so harmless a thing as the logical activity of the intellect? The answer, I think, is to be found in the fact that neither they nor their opponents realised quite how harmless it was. The empiricists were afraid of allowing even a subordinate capacity to the intellect for fear that it would rise up and overwhelm them; and what gave point to their fears was that the rationalists said that it quite certainly would. But in this they, for their part, were clearly mistaken. Many rationalist philosophers seem to have believed that because mind is required for knowledge of universals, mind must be a source of knowledge quite apart from the senses; but surely this is a very bad argument. If the intellect is concerned in a process of knowledge in its logical capacity, it does not follow that it will also have a function corresponding to that Kant describes under his "real use". Yet some eminent philosophers have thought the inference could be made, or so appearances suggest. Aristotle, for example, seems to think that because universals are present in sense-perception from the very first, a time can come when sense is wholly transcended; and the same error perhaps lies behind the Leibnizian view that sensing is only an inferior form of thinking.

Both Aristotle and Leibniz were philosophers who, like Plato and Descartes, Spinoza and Kant himself, maintained that the intellect has a real as well as a logical use, if the Kantian terminology can be adopted. I am not suggesting that they were wrong in this, but merely that so far as their claim was based on the facts which prove that the intellect has a *logical* use it must be adjudged quite worthless. To prove that the intellect has a real use, and is truly a source of knowledge independently of the senses, we must adduce quite different arguments. We must either show that men have (to adopt Kantian language once more) a faculty of intellectual intuition; or we must point to certain propositions which cannot be shown, even ultimately, to depend for their validity on sense-experience. Rationalists have, of course, made great use in the past of the propositions of mathematics for this purpose, and their argument has been, in principle, perfectly correct. Whether it is valid is, of course, another question, to which modern empiricists have their own answer.

The issue between rationalism and empiricism, then, turns on the

problem not of whether the human intellect has any function in knowledge, but of whether it has any function over and above its logical activity. That it has a logical function should be common to empiricists and rationalists. Perhaps I should mention before concluding, too, that theories which argue that the intellect has a real use can be of more than one type. We can distinguish them as more or less full-blooded. Philosophers who adopt the former alternative will hold that the mind is capable of knowing certain factual propositions about a sphere wholly transcendent of the world of sense-experience; and such a claim to what is often called "metaphysical" knowledge was in fact made by supporters of the Leibnizian school. But this full-blooded theory of the real use of the intellect is not the only possible theory. For we may argue, as Kant does in his mature philosophy, that the intellect (or, as he says, understanding and reason) is the source of certain very important positive propositions, but that these express no more than a certain necessary form which all objects falling within the sensible world must possess. Such propositions can then be described as prescriptive in character. According to this view, it may be pointed out, the intellect is, as in the logical use, to some extent subordinate to and dependent on the senses, for without sense-material to inform it is powerless; yet its function is clearly of far greater importance than would be the case if its activities were purely logical. I think myself that it is on the Kantian type of view of the real use of the intellect that empiricists ought to concentrate their attention. If they are to justify their thesis that the senses alone supply us with positive knowledge they must refute it; and if they are unable to refute it they must give up their thesis.

W. H. Walsh.

VII.—CRITICAL NOTICES.

Foundations of Ethics. The Gifford Lectures delivered in the University of Aberdeen, 1935-1936. By Sir W. DAVID ROSS. Oxford: Clarendon Press, 1939. Pp. xvi + 328. 15s.

THE Provost of Oriel's book, *The Right and the Good*, published in 1930, was much the most important contribution to ethical theory made in England for a generation. Every one interested in ethics had to take serious account of it, and it gave rise to a vast amount of critical discussion. The Provost has taken careful note of these criticisms, and he has devoted a considerable part of his Gifford Lectures to restating his position, with such modifications as he now considers necessary, and defending it against criticisms which he thinks invalid. This in itself constitutes a very valuable bit of work, but it is by no means the whole of the book. There is a chapter on ethical epistemology, one on the psychology of moral action, and one on indeterminacy and indeterminism, which treat elaborately subjects that are hardly touched upon in *The Right and the Good*; and the last two chapters, on "The Nature of Goodness" and on "Moral Goodness", contain a great deal of important new material.

In the Introduction Ross divides attempted definitions of ethical terms into "attitude-theories" and "consequence-theories", and then sub-divides each of these into a naturalistic and a non-naturalistic sub-class.

In chapter ii he discusses naturalistic definitions of "right". He rejects consequence-theories in this connexion as highly unplausible, and devotes most of his attention to attitude-theories and logical-positivist theories. In discussing attitude-theories he points out that, when A judges that X is right, he might, according to various possible forms of such theories, be judging either: (i) that A himself does (or would) contemplate X with a certain kind of emotion; or (ii) that all or most members of a certain class of men do (or would) do so; or (iii) that an agent, in doing such an act as X, would feel a certain kind of emotion about it. Ross thinks that the third is the most plausible form for a naturalistic attitude-theory to take. But he considers that there is a fatal objection to all such analyses, *viz.* that specifically moral approval or disapproval or a sense of obligation in acting is felt towards an act only in so far as the latter is already believed to be right or to be wrong on account of certain of its non-ethical characteristics.

Logical-positivist theories allege that ethical sentences *express* certain emotions or commands but do not have any specific *meaning*.

Ross discusses two forms of such theories, due respectively to Carnap and to Mr. Ayer. He rightly points out that they are not the results of direct reflexion on ethical facts, but are forced on their authors by their acceptance of a certain theory about the nature of significant sentences. In the main, therefore, he confines himself to criticising this general theory; but he also makes certain specific criticisms on the accounts of ethical sentences proposed by these two authors. Ethical sentences which refer to the past or to hypothetical circumstances or to the person who utters them cannot plausibly be supposed to express commands; and those ethical sentences which can be so regarded command an action on the ground of its rightness or forbid it on the ground of its wrongness.

Chapter iii is concerned with the nature of Rightness and of Obligation. The main points in it are the following: (i) Ross thinks that "right" is indefinable, whether naturalistically or non-naturalistically. (ii) An act is "obligatory" if (*a*) it is right, and (*b*) any alternative act in the circumstances would be wrong; but it must be admitted that "right", as applied to acts, is often used to mean the same as "obligatory". (iii) Strictly speaking, we ought not to use the word "obligatory" as if it stood for a property of certain acts, as, *e.g.*, the word "cruel" does. The only correct usage is to say that "so-and-so is under an obligation to act in such and such a way". (I think that what Ross has in mind here is true and important. But I think that it could be expressed in another way, *viz.* by saying that "obligatory" applies to *agibilia*, *i.e.* to possible acts, and that, when we say that a certain *agibile* is obligatory, this is a short way of saying that it ought to be enacted by some one.) (iv) Subject to this understanding, the only correct *application* of "obligatory" is to agibilia which it is in the power of a human being to enact or to refuse to enact at will. And the only correct *sense* of it is that which I have called the "deontological". (v) It is a mistake to suppose that "obligatory" applies only to agibilia which an agent has some inclination not to enact. "Duty is something that we ought to do *irrespective* of our inclination; not something that we ought to do in spite of a contrary inclination." (vi) Rightness is predicated of an act in relation to a situation and an agent, and this fact is brought out by calling it a species of fittingness or appropriateness. But its fittingness is quite unique, though it has a certain community with æsthetic fittingness.

Having decided that "right" is indefinable, Ross discusses in chapter iv the question whether ultimately all "right-making" characteristics, as I am wont to call them, can be reduced to a single one. At the beginning of the chapter all the theories which have already been dismissed as naturalistic *definitions* of "right" reappear in the new guise of accounts of the one and only right-making characteristic. Ross remarks that, whilst some of them were *prima facie* quite plausible in their former aspect, none of them are at all plausible in their latter aspect.

The various possible attitude-theories are dismissed fairly quickly, but more attention is paid to one of the causal theories, *viz.* the first half of Universalistic Ethical Hedonism, to wit the theory that being optimific is the one and only right-making characteristic. Ross argues that this proposition has no claims to be accepted unless it be self-evident, and he states that he does not find it so himself. He then adduces five objections to it. The first two are concerned with the rules for a right distribution of goods by an agent among other persons. There are certain principles which every one accepts here, and they cannot be justified on purely utilitarian grounds. The third is concerned with a similar fact about the distribution of goods by an agent between himself and others. The fourth objection is much more special, since it rests on the assumption that pleasure and pain can be equated and that they are the only intrinsic goods and evils. No doubt many utilitarians have accepted both these propositions, but they are certainly not an essential part of the doctrine which Ross is here concerned to refute. The fifth objection is that there are at least three kinds of special obligation, *viz.* to recompense those whom we have injured, to help those who have benefited us, and to keep our promises, which cannot plausibly be reduced to the single obligation of general beneficence as applied to these special kinds of situation.

Ross admits that a utilitarian might deal with the first two difficulties about distribution by distinguishing between primary and secondary goods, and holding that certain distributions of primary goods are themselves secondary goods, whilst other distributions of the same primary goods are secondary evils. He does not discuss the alternative which, I suppose, would be taken by really "tough" utilitarians, *viz.* that the only reason why those distributions which are commonly thought to be right are so is that they have greater "fecundity" than others. I do not think that this theory has the least plausibility. As regards the third difficulty about distribution, *viz.* that which concerns distribution between self and others, Ross argues that a utilitarian could not evade it by a distinction between primary and secondary goods. The fifth objection, about special obligations resting on special relationships, is elaborately discussed, so far as concerns promises, in chapter v.

Chapter v begins with a statement and an elaborate criticism of Mr. Pickard-Cambridge's strictures on the theory of *prima facie* obligations and of his utilitarian account of the ground of the obligation to keep one's promises and to return benefits. This is followed by a brief critical account of the theory of M. Katkov, a follower of Brentano, on the same subject.

Ross's own account of promise-keeping may be summarised as follows: (i) Strictly speaking, one is under an obligation only to do one's best to bring about a result which one has promised; and this is always understood by both parties. (ii) If, before the time for fulfilling the promise has come, circumstances have rendered it im-

possible for the promiser to fulfil it, it ceases to be a *prima facie* duty even to try to do so. But the changed circumstances may impose other *prima facie* obligations on the promiser. (iii) Suppose it has become much harder, but not impossible, to keep the promise. Then the *prima facie* obligation to try to do so is not diminished. But the changed circumstances may have given rise to new *prima facie* obligations, or may have increased the urgency of old ones, which conflict with that of keeping one's promise. So that the least unfitting act which the promiser can do in the new circumstances may be to break his promise. (iv) The obligation to keep a promise is annulled if the promiser has reason to believe that the promisee no longer wants the promised act done.

Ross admits that this last fact seems to support the utilitarian's contention that the ground of the obligation is the promisee's future gratification or disappointment. His answer is that we feel a different and a much weaker obligation to confer an unpromised benefit of the same amount, and that we do not consider ourselves to be released from our promises to a man when he dies, even if we do not believe in human survival. He admits that this might seem to be explicable by the persistence of a superstitious emotion in the promiser, but thinks it enough to answer that we should have a similar feeling if the promisee had merely gone abroad and was certain not to return. I doubt whether any utilitarian would need to feel much anxiety about either of these arguments as it stands.

Ross ends the chapter with the interesting remark that utilitarians have tended rather to over-estimate the disastrous consequences of promise-breaking in order to square their theory with the very strong sense of obligation which ordinary men feel in this connexion. I think that this is true ; but I also think that they might have secured their object without this exaggeration if they had laid more stress on the strength of the temptation to break one's promises, and therefore on the importance, from a utilitarian point of view, of a strong superstitious taboo being attached to promise-breaking.

Chapter vi is primarily concerned with the question whether the rightness or wrongness of an act is, in part at least, dependent on the nature of the motives which influenced the agent in doing it. Ross reiterates the doctrine, already asserted in *The Right and the Good*, that there is no such dependence. He considers separately the case of an act being done because the agent believes it to be right and desires to act rightly, and the case of an act being done from some other motive. He argues that, in the first case, to make the rightness of an act depend on the agent's motive would involve a vicious infinite regress ; in the second case it would involve the absurdity that the agent is under an obligation to have certain beliefs and desires and to be moved by them, although admittedly all this is completely out of the control of his will at the time when he is about to act. Finally, he argues that, if an act cannot owe its rightness to being done from the conscientious motive, which is admittedly the

highest possible one, it is most unlikely that it could owe it to being done from any other motive. These dialectical arguments are further supported by an appeal to the empirical fact that, when one is trying to decide what one ought to do in a given situation, one considers the nature of the possible alternatives, their probable consequences, and their relations to various factors in the external situation, but never the motives from which one would be acting.

As this opinion has been questioned, Ross devotes a great part of the chapter to considering the doctrines of Mr. Joseph, of Prof. Field, and of Prof. Reid, each of whom has, in one way or another, held that rightness of an act can depend on goodness of motive in doing it. Mr. Joseph's theory on this subject is part of a more general theory about the connexion between rightness and goodness, and Ross discusses the other parts also. I should be inclined to put a different interpretation from Ross's on the passage from Mr. Joseph's *Some Problems in Ethics* quoted on page 131 of the present work. But anyone who reads that passage will admit the extreme difficulty of translating it from Josephan into English.

In chapter vii Ross discusses two closely connected questions: (i) Of what kind of change can it be said, in the strictest sense, that a person ought or that he ought not to produce it? (ii) What bearing, if any, has an agent's ignorance or error about (*a*) non-ethical facts, and (*b*) ethical facts, on what he ought to do? Questions (i) and (ii, *a*) have been elaborately discussed in Prof. Prichard's lecture on "Duty and Ignorance of Fact". Ross gives a synopsis of this lecture and states that he has been converted by its arguments. The answer to question (i) is that, in the strictest sense, a person cannot be under an obligation to produce any change which is not wholly within his power. Now the only change which it is wholly within an agent's power to produce is that mental change which Prichard calls "setting oneself to perform" an action. Whether this will produce the expected overt movements of one's own body depends on conditions which are out of one's power, though they are in fact generally fulfilled. And whether these bodily movements, if they take place, will produce the intended changes in the external world depends on conditions which are not only out of the agent's power but also may easily fail to be fulfilled. Hence, strictly speaking, no one is under an obligation to make any particular bodily movement, and *a fortiori* no one is under an obligation to make any particular change in the external world. Similar remarks would apply, *mutatis mutandis*, to changes in the agent's dispositions, beliefs, emotions, etc. This argument appears to me to be quite conclusive.

Granted that nothing but "self-exertions", as Ross calls them, are obligatory on an agent, and granted that of all the alternative possible self-exertions in a given situation the agent ought to make that one which is "right", there remains an ambiguity in the latter word. In one sense of "right" the right self-exertion in a given situation is that one which *will in fact* produce the most claim-

fulfilling action open to the agent in the situation *as it actually is*. This may be called the *completely objective* sense of " right ". Now the agent may be partially ignorant or partially misinformed about the nature of the situation and about the effects, direct or remote, which various alternative self-exertions will produce. There is another sense of " right " in which the right self-exertion is that which *would* produce the most claim-fulfilling action *if* it had the consequences which the agent expects it to have and *if* the situation were as he believes it to be. This might be called the *factually subjective and ethically objective* sense of " right ". Ross agrees with Prichard in holding that the self-exertion which is right in the completely objective sense is *not*, as such, the one which the agent ought to make. Is the obligatory self-exertion the one which is right in the factually subjective and ethically objective sense ? Ross holds that this is not necessarily the case.

The agent may be ethically ignorant and ethically mistaken. He may fail to see that some of the factors which he believes (rightly or wrongly) to be present in the situation would impose certain claims upon him. He may be mistaken about the relative urgency of the various claims which he rightly believes that the situation would impose on him if it were as he believes it to be. There is a third sense of " right " in which the right self-exertion is that which the agent, in the light of his non-ethical beliefs about the nature of the situation and the consequences of his self-exertions, and in the light of his ethical beliefs about the claims which the situation as he sees it imposes on him, *believes* will produce the most claim-fulfilling action. This may be called the *completely subjective* sense of " right ". Ross holds that the self-exertion which is right in the completely subjective sense is the one which is, as such, obligatory on the agent. It may, of course, happen to coincide with the one which is right in one or other of the two remaining senses ; but, if so, this is irrelevant to its being obligatory.

Ross produces various plausible reasons for accepting this view if it be a possible one. But he sees that it is *prima facie* open to the fatal objection that it seems to make being right logically dependent on being thought to be right. To this Ross makes the following answer. That self-exertion which it is right for a given individual to make at a given moment is the one which *is* most fitting to his *total* state of belief, ethical and non-ethical, at the time. What the theory asserts is that the self-exertion which has this property is the one which the agent *believes* to be most fitting to the *non-ethical part* of his total state of belief. There is nothing logically vicious in the statement that the former property is entailed by the latter. Nor is there anything very paradoxical in it when we remember that the only thing that anyone is, strictly speaking, under an obligation to do is to make a self-exertion, and that this is simply to make a certain kind of change in the state of his own mind. It is not surprising that the obligation to make a certain change in one's own state of mind

should depend entirely on the characteristics of certain other states of one's own mind.

Ross continues to hold that the moral goodness or badness of an act depends only on the nature of the agent's motive in doing it, and that being done from a sense of duty is a good-making characteristic of an act. It follows at once that an act which is made good by this motive is also a right act, in the completely subjective sense of "right". The converse does not hold, since an act which is right in this sense may be done from some other motive than the desire to do what is right as such. Lastly, an act which is rendered good by some good-making motive other than the conscientious motive may not be right in any sense of the word.

Chapter viii is concerned with epistemological questions about ethical cognition. The main points are as follows: (i) Though rightness always depends on the presence of certain right-making characteristics which entail it, it is not the case that singular judgments of rightness are always reached by inference from general principles which assert such entailments. On the contrary, the general principles must have been reached by intuitive induction from particular cases in which the compresence of rightness with certain non-ethical characteristics was directly observed. (ii) We can *know*, in regard to certain features which frequently recur in practical life, that they impose claims on us to act in certain ways. But, except within wide limits, the relative urgency of these claims is a matter of *opinion*. (iii) A person can never know what is the right act in the completely objective sense. If he is subject to several conflicting obligations, he cannot know what is the right act in the ethically objective and non-ethically subjective sense. But he can form a reasonable belief on this point. When he has done so, he *knows* what is the right act in the completely subjective sense; since he knows what his own belief is, and nothing further is needed. (iv) Since general beneficence is one of our *prima facie* duties, though not our only duty as utilitarians hold, we shall often have to make calculations about the goodness or badness of the consequences which our acts are likely to have. This leads Ross to consider the nature of goodness as a magnitude and the nature of our estimates of it.

The discussion on this last point seems to me to fall considerably below the very high level of the rest of the book. I cannot go into details, but I consider the argument on pages 180 to 183 (in which a theory of Prof. Price's is being criticised) to be curiously naïve and (for Ross) somewhat obscure. We are not told on what principle Ross claims to be ordering goods in respect of magnitude at the bottom of page 181 and the top of page 182. After struggling with this passage for a long time, I came to the conclusion that the following rule covers all the cases about which Ross says that comparison is possible and none of the cases about which he says that it is impossible on Price's theory. Let X and Y be two goods, of which at least one is an aggregate of several goods. It is assumed that no two goods in

the same aggregate are equal. Then X is certainly preferable to Y if, and only if, one or other of the following two conditions is fulfilled. *Either* (*a*) X and Y both contain the same number of goods, and, for all values of *r*, the *r*-th best good in X is better than the *r*-th best good in Y; *or* (*b*) X contains more goods than Y, and there is a part U of X such that condition (*a*) is fulfilled with U substituted for X.

The great defect is the absence of any adequate discussion of the conditions under which several co-existing goods constitute a whole which has some amount of goodness.

In chapter ix Ross deals with the psychology of moral action. An action may be started either (*a*) by one's deciding on an end and then taking means to bring it about; or (*b*) by an action being suggested to one, either by another person or by something in one's environment, whereupon one goes on to consider what effects it is likely to have. Men may be divided roughly into "Planners", and "Suggestible Persons". The former may, in particular cases, be less estimable than the latter; but it is the former who become Provosts while the latter remain Professors.

Ross gives an excellent analysis of planned action. In deliberating about alternative ends a person does consider in rough outline the means which would be necessary to attain each of them, and he does not decide to pursue a certain end unless he assumes that it could be reached by some means or other which are not too discreditable or too unpleasant. When we make a decision, what we decide is to take whatever means may be necessary to reach a certain one of the ends about which we have been deliberating. Then there follows a stage of more detailed reflexion about alternative means to the chosen end. At this stage we have to consider and compare a number of alternative causal chains, each starting from a possible self-exertion and terminating with the attainment of the chosen end. Each link in any such chain has to be appraised in respect of (*a*) its efficacy as a causal ancestor of the chosen end, (*b*) its intrinsic attractiveness or repulsiveness, (*c*) the likelihood of its being a causal descendant of any self-exertion that we can make, and (*d*) the attractiveness or repulsiveness of its causal ancestors and its causal descendants. Finally, there comes the act of self-exertion which brings into being the first link in the chosen means to the chosen end. This is always different from the act of decision, and it may not begin until long after the latter has happened.

The chapter ends with a discussion of conscientious action, in which Ross expresses his dissent from Kant's view that such action is not caused by desire but by a special kind of emotion. This view of Kant's arose from his holding an unreasonably narrow and depreciatory opinion about desire.

The tenth chapter deals with indeterminacy and indeterminism. It opens with a discussion of indeterminacy in physics, and introduces a refreshing breeze of good sense into a region in which

eminent scientists, with a taste for philosophy and no training or aptitude for it, have talked great nonsense whilst the public have "wondered with a foolish face of praise". Ross regards the law of universal causation as self-evident; and rightly insists that the only question here that concerns philosophy is whether this is so or not, and that no discoveries in practical or theoretical physics have the slightest bearing on it. Now the law of causation is a proposition about events, *as such*, and not about physical, as opposed to mental, events. Moreover, acts of decision and of self-exertion, however peculiar they may be in other respects, are events. Therefore Ross consistently accepts determinism about human voluntary action. Whether we accept Ross's view of the law of universal causation or not, I think that there can be no doubt (*a*) that there is a very strong *prima facie* case for it; (*b*) that, if it be accepted, his conclusion about determinism in human voluntary action follows; and (*c*) that, except as a consequence of this *a priori* premise, such determinism is not plausible enough to be worth serious discussion.

The rest of the chapter discusses the *prima facie* objections to determinism based on the alleged intuition of freedom and on the contention that certain ethical facts entail freedom. The discussion is excellent, but the subject is a hackneyed one, and I decline to pander to sadistic readers by presenting them with the spectacle of Ross flogging these moribund asses.

We revert to more wholesome pleasures in chapters xi and xii, which are concerned with the nature of goodness in general and of moral goodness. These contain much the best discussion of this subject that I know of. The results may be summed up as follows:—

(i) The word "good", in a non-instrumental sense, is commonly applied to (*a*) certain moral volitions, emotions, actions, dispositions, and characters, *viz.* to such as would be called "benevolent", "conscientious", "courageous", and so on; (*b*) certain intellectual and æsthetic dispositions and actions and characters, *viz.* to such as would be called "intelligent", "æsthetically creative", and so on; and (*c*) to pleasures. The question is whether the word "good" has the same meaning in these various applications. (ii) Ross holds that, in applications (*a*) and (*b*), being good entails and is entailed by being a *fitting* object of *admiration*. (iii) This shows that "good", in this sense, does not apply to pleasure; for it is obvious that no one thinks it appropriate to *admire* a pleasant sensation, as he feels it appropriate to admire a benevolent action or a process of cogent reasoning. (iv) In the case of pleasure a distinction must be drawn between a person's own pleasures and those of others. *Other* men's pleasures are, as such, fitting objects of *sympathetic satisfaction* to anyone, provided that they are morally innocent. And this is what is meant by calling them "good". But a man's own pleasures cannot, even when they are innocent, be called "good" by him, even in this sense. (v) When a person calls his own pleasures "good" he is not ascribing any characteristics

to them. He is merely expressing a " pro-attitude " towards them. (vi) In all three applications the use of the word " good " expresses a pro-attitude towards the object to which it is applied, and this is the only feature common to all three. (vii) Anything that is a fitting object of admiration, and therefore good in the first sense, is also a fitting object of sympathetic satisfaction to another person, and therefore good in the second sense. But the converse does not hold. (viii) Goodness, in the second sense, is a relational property ; but it is not a naturalistic one, since fittingness is not so. (ix) " Good ", in the first sense, does not *mean* the same as " fit to be admired ", any more than " being equilaterally triangular " means the same as " being equiangularly triangular ". For the sense in which " admiration " is here used is such that to admire anything means to feel a certain kind of emotion towards it which can be felt only in respect of the supposed *goodness* of the object. " Good ", in this sense, is the name of a non-natural quality, whose supposed presence in an object makes us admire it, and whose actual presence is necessary and sufficient to make it a fit object of admiration.

I am sure that Ross is right in refusing to admit that pleasure is good in the sense in which virtue and intelligence are so ; and I am sure that he is right in distinguishing a man's own pleasures and those of others in respect of goodness. But I feel less confident about some of the details. It seems to me, in the first place, that, on Ross's theory, each man would have a *prima facie* obligation to give himself as many innocent pleasures as possible, since each of them would be a good (in the second sense) for every one but himself even if no one else happened to know about them or to feel sympathetic satisfaction in contemplating them. Yet I understand Ross to hold that no one has any obligation to give himself pleasures, and this is certainly in accordance with common sense. Again, I am not greatly impressed by the argument to show that " good ", in the first sense, is the name of a non-naturalistic quality. I should have thought that the following account of the facts would be about equally plausible. Admiration, in the sense required, is an emotion which is appropriate to the presence of any one of a large number of natural characteristics, such as courageousness, truthfulness, etc. " To be good ", in the first sense, simply means " to have one or other of the natural characteristics which make an object fit to be admired ". And so each of these natural characteristics counts as a good-making characteristic. I cannot see the need to have both a non-natural quality of goodness, grounded on these various natural characteristics, and a non-natural relation of fittingness grounded on this non-natural quality.

Chapter xi ends with a discussion of what Herr N. Hartmann calls " *Sachverhaltswerten* ". These are goods which are not events in or dispositions of persons but are states of affairs involving certain relations between persons. It has been suggested that utilitarianism might be saved by the recognition of such goods. Ross takes the

view that these states of affairs are good in the sense in which other men's pleasures are so, *viz.* that they are fitting objects of sympathetic satisfaction to anyone who contemplates them. But he holds that some at least of them fulfil this condition only because they are fulfilments of a pre-existing obligation, *e.g.* a promise. In such cases the rightness of the act which brings the state of affairs into being cannot be derived from the goodness of the state of affairs.

In chapter xii, on Moral Goodness, Ross considers first the bearers of moral goodness, and decides that these are (i) voluntary actions proceeding from certain motives, (ii) desires which would lead to such actions if not prevented by other causes, (iii) certain emotions, (iv) habits and dispositions to do good voluntary actions and experience good desires and emotions, and (v) character, in so far as the various good factors in it are present in intensities commensurate with their goodness.

Next Ross criticises Butler's account of particular propensities, and decides that in most cases we desire activities, such as eating, not for themselves, but because we have found them to be pleasure-giving or pain-relieving and expect them to be so again in future.

He then gives a list of fourteen important kinds of possible motive, consisting of seven pairs ; each pair consisting of a highly restricted and a highly generalised motive of the same kind, *e.g.* desire that a certain person's character may be corrupted in a certain respect and desire for the general corruption of all rational beings. Certain of these motives are devilish and not human, others occur only in a few exceptionally bad or exceptionally mad human beings, but most of them are present in some degree in all normal men. Ross then arranges the non-devilish motives in a moral hierarchy, ranging from desire to corrupt another man's character, at the depth of human badness, to the desire to do on all occasions that act which is most fitting on the whole, at the height of human goodness. Finally, he discusses mixture of motives, and draws the essential distinction between the case where admixture of a lower motive was *necessary* to ensure the doing of a certain action and the case where such a motive, though present and co-operating, was causally superfluous. He points out that the goodness or badness of an action is not determined wholly by the agent's motive *for* doing it. This may be neutral or good, and yet the action may be bad. We have to take into account also the wrong-making factors in the action, which the agent noticed and by which he was not enough repelled, and the right-making factors in alternative actions, which he noticed and by which he was not enough attracted.

The book ends with an admirable summary of the preceding argument. In the last two pages the Provost relieves the Gifford Trustees from all imputation of breach of trust by referring in civil terms to his Creator.

Philosophical writing, at the present time, is in rather an unfortunate state. Those who can think clearly and write lucidly are

mostly Logical Positivists, assiduously occupied in taking in each other's poverty-stricken washing; whilst those who are concerned with matters of human interest and importance are too often confused or crazy. It is therefore an immense pleasure to read a book like these Gifford Lectures of Ross's, in which good sense and acuteness and clarity, commonly lavished on trivial straw-chopping, are devoted to elucidating questions of perennial significance. I hope that generations of undergraduates, in the intervals between making the world safer and safer for democracy, will come to know and appreciate this book under the affectionate and accurate nickname of "*The Righter and the Better*".

C. D. BROAD.

Die gegenwärtige Lage in der mathematischen Grundlagenforschung. Neue Fassung des Widerspruchsfreiheitbeweises für die reine Zahlentheorie. (Forschungen zur Logik, etc., Neue Folge, Heft 4.) By GERHARD GENTZEN. Leipzig: S. Hirzel, 1938. Pp. 44. M. 2.20.

Grundlagen der Mathematik, Bd. II. By D. HILBERT and P. BERNAYS. Berlin: J. Springer, 1939. Pp. xii + 498. M. 43.80.

BOTH of these books on the foundations of mathematics are written from the formalist standpoint. 'Book', however, is a misleading word to apply to Dr. Gentzen's survey *cum* specimen of original research. Philosophers will find more of direct interest in the report on the present state of research in this subject which fills the first thirteen pages of the pamphlet. Here a tremendous range of topics is taken at the gallop; the mathematical paradoxes, the mathematical notion of infinity, the relations between the various schools of research in the foundations of mathematics, interpretations of some of the more remarkable theorems and some comments on the use of non-finite methods in formalist proofs make a breathless and tantalisingly brief appearance. While the ground covered coincides very nearly with the second chapter of Heyting's useful report, which appeared in 1934 (*Mathematische Grundlagenforschung, Intuitionismus, Beweistheorie*), the later pamphlet, for all its compression, contains a few novelties of emphasis. It is interesting, for example, to find attention drawn to the importance of Skolem's theorem, according to which "Wenn zu einem Axiomensystem von bestimmter Art überhaupt ein Modell, beliebig hoher Mächtigkeit, existiert, so existiert auch bereits ein abzählbares Modell, welches das Axiomensystem erfüllt" (p. 11). [There is no mention of this theorem in Heyting; Hilbert and Bernays, whose book will be henceforward quoted as '*G.M.*', attribute priority to Löwenheim (1915), and state that the result was used by Skolem himself in 1920, two years earlier than the date quoted by Gentzen (*cf. G.M.*, 2, 182-183).] That *every* system of axioms can be satisfied by a denumerable (*abzählbar*) set of objects,

e.g. by the set of positive integers, seems a highly paradoxical statement. How is it reconcilable, for instance, with Cantor's well-known diagonal proof of the *non*-denumerability of the set of decimals between 0 and 1 ? What exactly is meant, in the enunciation of the Löwenheim-Skolem theorem, by saying that the axiom system in question can be satisfied (*erfüllt*) by a denumerable set ? For the answers to such questions as these one will naturally turn to Hilbert and Bernays' magisterial record of the aims, methods, and achievements of the formalist school. Since their treatise culminates in a discussion of the proof by Gentzen (1936) which is presented in a simplified and improved version in the second part of his own pamphlet, it will be convenient to interpolate here an outline of the contents of the *Grundlagen der Mathematik* before returning to consider in detail Gentzen's striking contribution to the formalist programme.

The philosopher who assumes that the title of *Grundlagen der Mathematik* describes a modern version of *Principia Mathematica*, written from the formalist standpoint, will be disappointed. The character of the formalist programme (as further explained below) demands that philosophy should be treated with the frigid politeness accorded to a gate-crasher. The generous scale of the treatise is accounted for by the desire to achieve accuracy in the detailed discussion of technical methods, often published here for the first time, and always involving notions whose exact definition requires meticulous attention to detail. Thus although the primary object is to present proofs of consistency (*Widerspruchsfreiheit*) of mathematical systems, no such proof appears in the first volume (reviewed in MIND, XLIV, 392 (1935)), whose 468 pages are occupied by preliminary considerations, such as the possibility of transforming formulæ in the propositional calculus into a standard form, the extent to which logical quantifiers can be eliminated from an axiom system, the exact specification of the rules for the use of recursive definitions, and proofs of categoricity (*Entscheidungsbarkeit*) and completeness (*Vollständigkeit*) for simple systems. The new volume, where we reach proofs of consistency for the first time, is by far the more interesting.

The first three chapters consist mainly of an investigation into the elimination of apparent variables with the aid of Hilbert's ϵ-symbol. There follows the application of the results so obtained to the examination of various axiom systems, culminating in a critical discussion of the fundamental theorems discovered by Löwenheim, Skolem, and Herbrand. The remaining two chapters are of particular interest: in chapter 4, the method of arithmetising axiom systems, used to such effect in Gödel's work, is fully explained, with examples; in chapter 5 (" Der Anlass zur Erweiterung des methodischen Rahmens der Beweistheorie ") the connexion of Gödel's theorem with the semantic paradoxes is examined and Gentzen's proof is outlined (though less fully than in his own pamphlet). Four long supplements conclude the work: (1) A summary of the various terms and symbols

employed in the text, designed to make the second volume independent of the first, (2) more precise discussion of the notion of computable function (berechenbare Funktion) and the researches in which it occurs, (3) some supplementary theorems on the structure of the propositional calculus, (4) an outline of the symbolism used in presenting pure mathematics as a deductive system (" Formalismen zur deduktiven Entwicklung der Analysis "). A fairly detailed analytical table of contents is provided, but the index is sketchy and there is no bibliography (although numerous bibliographical footnotes are scattered throughout the text).

The section which will undoubtedly arouse most interest is that on Gentzen's work. Its significance can be appreciated only against the background of the history of the formalist programme.

It has been taken for granted during the last few years that the formalist programme could not survive the discovery in 1931 of Gödel's theorems on the incompleteness of deductive systems. Hilbert's original programme (outlined in 1900 and definitely presented in 1923) was largely provoked by the now familiar paradoxes in logic and the more rarefied portions of pure mathematics. (It is interesting to note that discussion of these paradoxes continues as vigorously and as inconclusively to-day, *cf. e.g.* MIND, XLVIII, 191.) Unable to accept Russell's theory of types or Brouwer's still more revolutionary proposals for the reform of mathematics, and firmly convinced, like most creative mathematicians, of the freedom from contradiction of those parts of mathematics to whose advancement he had contributed, that is, of at least the theory of functions, Hilbert proposed his ingenious method for defending mathematics, by the use of its own technical methods, against the damaging criticism of the philosophical logician.

The basic idea of the method (as satisfying in its economy of means as the most elegant of geometrical artifices) consisted in using mathematical methods, undiluted by the admission of the less formal methods of philosophical argument, to establish the consistency of mathematics itself.

The starting-point was to define consistency or freedom from contradiction (*Widerspruchsfreiheit*) in mathematical terms and so to make it as amenable to mathematical manipulation as, say, elasticity or gravitation. (This was, of course, the familiar replacing of a qualitative notion by an equivalent structural notion which is the prelude to all application of mathematical methods.) In order to do this it was first necessary to be provided with a complete symbolism for mathematical notions and the logical operations to which they are subjected. By 1913, Russell and Whitehead, building on the work of Peano's *Formulaire*, had done this in essentials. Here the contribution of Hilbert and his followers consisted partly in simplification of notation (*e.g.* by deleting class-symbols and the axiom of reducibility), partly in further changes in symbolism (notably the use of Hilbert's ϵ-symbol in place of apparent variables)

made with an eye to subsequent technical difficulties, partly in the stricter formulation of notions relatively neglected by Russell and Whitehead (*e.g.* those of substitution and the permissible forms of definition). But these changes introduced no innovations of principle.

The fact that Peano, Whitehead, and Russell had thus provided an adequate symbolism for representing all specific mathematical terms, complexes, and the logical operations to which they were subject, allowed the formalists to regard every mathematical system as a *calculus*. That is to say, as a system of classes of *marks* (the 'axioms') manipulated, in deliberate neglect of any meaning which the marks might otherwise have, in accordance with rules taking into account only the shape and arrangement of the marks. Any mark so obtained is a 'theorem' of the system; any series of marks obtained by successive application to axioms of the rules of the system is a 'deduction' or 'proof'. *Widerspruchsfreiheit* is then definable as a certain (negative) property of the calculus, *viz.* that none of the theorems shall be of the form $0 \neq 0$. (It is easily shown that this is equivalent to the more natural definition of consistency as the impossibility of the calculus simultaneously containing theorems of form p and $\sim p$ respectively.) The investigation of calculi, in this light, independently of all regard for the 'meaning' of the component complex marks, was christened *metamathematics*.

Now a metamathematical inquiry into the consistency of mathematics, such as Hilbert proposed, has to satisfy one essential condition if the whole procedure is not to involve a vicious circle. The methods *used* in such an inquiry (as contrasted with those embodied in the calculus examined) need not, and in general will not, be adequately symbolised; they will normally be communicated by colloquial language supplemented by occasional symbols defined *ad hoc*. Let the methods used in a metamathematical inquiry be distinguished as the *informal* methods (themselves able, of course, to form the subject-matter of yet another metamathematical investigation). The condition referred to at the outset of this paragraph is that the non-formal methods used shall be simpler than the methods embodied in the calculus examined. And, since simplicity is not easily definable, it is well that the informal methods should be of minimal simplicity. In practice, the mathematical paradoxes and the consequent suspicion as to the validity of certain mathematical methods, turn upon the use of the "actual infinite" (or, as Gentzen calls it, the "an-sich-Auffassung" of the infinite). The actual infinite is involved in every assertion about an infinite set whose verification would require, *per impossibile*, the scrutiny of each member of the set, *i.e.* in every assertion whose truth or falsity does not follow from the defining law of the infinite set to which it refers. (A definition of the 'actual infinite', not attempted by Gentzen, would be difficult; but *cf.* Alice Ambrose, MIND, XLIV, 186 *et seq.*) A prudent formalist will exclude this debatable notion from his

' informal ' procedures if he can. He will use only ' finitist ' methods, such as are discussed in *G.M.*, 1, 20-42. The difficulty of delimiting the scope of finitist methods in a fashion non-arbitrary and widely acceptable to all schools of thought in the foundations of mathematics has in the past reinforced the tendency of formalists to err on the side of safety by using the very simplest methods.

A favourite mode of attacking the problem of demonstrating the *Widerspruchsfreiheit* of a system, which will illustrate this ascetic tendency of formalist technique, consists in finding a way to convert each valid deduction (regarded as if presented for examination in all specificity) terminating in the figure $0 \neq 0$, *in a finite number of steps* into another deduction, equally valid, in which an inconsistent *premiss* has been employed. In this way the freedom from *overt* inconsistency of the axioms (absence of the figure $0 \neq 0$) would ensure the consistency of the entire system. A proof of this character uses methods simple enough to be above suspicion.

The severe restrictions of permissible techniques which Hilbert's programme imposed upon its followers largely accounts for the disappointing results obtained in the period from 1922 onwards. Nobody can read *G.M.* (or, better still, the original papers) without a growing respect for the ingenuity with which early formalist writers struggled to overcome the crippling severity of their self-imposed asceticism. Proofs of the consistency of the most elementary branches of pure mathematics were, it is true, eventually provided, but as recently as 1934 no such proof was available for a system in which, say, Fermat's theorem could be expressed. And if there seemed little hope of establishing the consistency of an axiom system embodying the properties of integers, the prospects of doing as much for the theory of functions (the ultimate aim of the whole programme) seemed discouragingly remote.

The theorem of Gödel (1931), which has already been mentioned, gave the theoretical explanation for this ' fiasco ' (Bernays : " . . . die zeitweilige Fiasko der Beweistheorie ", *G.M.*, 2, vii). Suppose a calculus is sufficiently well furnished with symbols to be able to formulate the properties of integers (*i.e.* to include Peano's axioms with a principle of mathematical induction, and provision for the recursive definition of the addition and multiplication of integers). Then it is impossible, Gödel proves, to demonstrate the consistency of such a calculus (in the formalist way) if the informal methods used are simple enough to be symbolised in the calculus itself. From such a ' proof ', presented in all specificity, it would be possible to construct, by Gödel's method, and in a finite number of steps, a specific inconsistent conclusion in the original calculus ! This was proved in a ' finitist ' way ; thus Gödel had destroyed the formalist hopes with their own weapons.

It was adding injury to insult to use the very methods of a programme designed to establish the authority of mathematical method in order to demonstrate unsuspected theorems (of which that which

now concerns us is only a sample) upon the limitations of mathematics. The only hope which remained was that the cumulative complexity of Gödel's work might conceal a blunder.

The major importance in the formalist programme of Gentzen's work (first published in 1936 and now presented in an improved and simplified form in the second part of his tract) lies in the possibilities it offers to formalists of evading the destructive implications of Gödel's conclusions.

Once the correctness of Gödel's results (now no longer in dispute) is accepted, it follows that proofs of consistency for " die reine Zahlentheorie " (defined by Peano's axioms, induction and definitions of multiplication and addition, as above), or more complex systems, must use informal methods which are (*a*) incapable of representation within the calculus, and yet (*b*) still " finitist ".

Since present conditions make Gentzen's papers even more inaccessible than they would normally be to an English-speaking public, and in view of what promises to be the permanent importance of his work, it may be useful to outline his methods.

The calculus which he examines, more complex than any yet considered by formalists, is equivalent to that defined by the following axioms (*cf. G.M.*, 1, 371) :—

$$\begin{gathered} a = a \\ a = b \rightarrow (\mathrm{A}(a) \rightarrow \mathrm{A}(b)) \\ a' \neq 0 \\ a' = b' \rightarrow a = b \\ a + 0 = a \\ a + b' = (a + b)' \\ a \,.\, 0 = 0 \\ a \,.\, b' = a \,.\, b + a \\ \mathrm{A}(0) \;\&\; (x)\,(\mathrm{A}(x) \rightarrow \mathrm{A}(x')) \rightarrow \mathrm{A}(a) \end{gathered}$$

where ' *a* ', ' *b* ' are free variables, whose values are ' 0 ', ' 0′ ', ' 0″ ', . . . ; ' *a*′ ' may be understood as the immediate successor of ' *a* ' in the series of integers ; ' A ', ' B ' as free functional variables of type function-of-integer-argument ; and ' . ' and ' + ' as the arithmetical operations of multiplication and addition respectively. In addition, it is necessary to suppose axioms of the propositional calculus and rules for their transformation appended (*cf. G.M.*, 1, *passim*, but especially 45-158 and 286-379). Let the whole define the system ' Z ', say.

Z is complex enough for Gödel's theorem to apply : Gentzen therefore has to find a method (in practice a way of arranging all the deductions of Z in a linear order) which cannot itself be symbolised in Z. He does this in effect by assigning to each deduction a unique transfinite ordinal number as a preliminary to establishing a required property of all the deductions by the application of ' transfinite induction ' to their associated ordinals. Since transfinite ordinals cannot be defined in Z, Gentzen's method cannot be used in Gödel's

way to establish a contradiction in Z. But it must appear highly paradoxical to call such a method, which borrows notions from the debatable realm of Cantor's ideas, " finitist ". It is worthwhile therefore to give an alternative formulation (not Gentzen's own) from which the notion of " transfinite ordinal " is absent.

Let us forget about transfinite ordinals and define instead a series of ' patterns ', a word whose significance is to be exhaustively delimited by the following cumulative definitions :—

1. Each pattern is constructed by using the symbols ' (', ') ', ' 0 ', allowing each to be repeated a finite number of times.
2. Each pattern belongs to one of a series of classes, C_1, C_2, . . ., C_n, . . ., n being a positive integer.
3. C_1 consists of a series of patterns of which the following are the first five members :—

   0 (0) (00) (000) (0000)

   (*i.e.* each member of C_1 consists either of ' 0 ' alone or else of a finite run of ' 0 's enclosed in brackets).
4. In C_1 a pattern is said to *precede* another pattern, if, and only if, the number of ' 0 's enclosed in brackets in the first is less than that in the second. (Thus ' 0 ', having *no* ' 0 's enclosed in brackets precedes every other pattern in C_1.)
5. C_2 consists of ' 0 ' and all symbols of the form

   $$(a_1 a_2 a_3 \ldots a_k)$$

   where (i) each ' a ' belongs to C_1, (ii) k is finite, (iii) a_2 is either identical with, or precedes, a_1 in C_1, a_3 is either identical with, or precedes, a_2 in C_1, . . ., a_k is either identical with, or precedes, a_{k-1} in C_1.
6. Of two patterns in C_2, say $(a_1 a_2 a_3 \ldots a_k)$ and $(b_1 b_2 b_3 \ldots b_l)$ the latter is said to precede the former in C_2, if, and only if, (i) the first ' b ' which is not identical with the corresponding ' a ' precedes it in C_1, or (ii) all the ' b 's are identical with their corresponding ' a 's and $l < k$. [*E.g.* ((000)(00)0) precedes ((000)(00)(0)).]

In the same way we can define C_3, C_4, . . . and the relation of precedence between patterns in each of these classes.

7. In general, C_n, where ' n ' is a positive integer, consists of ' 0 ' and all patterns of the form

   $$(a_1 a_2 a_3 \ldots a_k)$$

   where (i) each ' a ' belongs to C_{n-1}, (ii) k is finite, (iii) a_r is either identical with, or precedes, a_{r-1} in C_{n-1}, for each value of r from 2 to k.
8. Of two patterns in C_n, say $(a_1 a_2 a_3 \ldots a_k)$ and $(b_1 b_2 \ldots b_l)$ the latter is said to precede the former in C_n if, and only if, (i) the first ' b ' which is not identical with the corresponding ' a ' precedes it in C_{n-1}, or (ii) all the ' b 's are identical with their corresponding ' a 's and $l < k$.

It is easily seen that, for all values of n, C_n includes all members of C_{n-1}. If a pattern belongs to C_n but not to C_{n-1}, let its *rank* be called n, and let the rank of all patterns in C_1 be 1.

The effect of the above (recursive) definitions of pattern, precedence, and rank (which are a great deal less complicated than the space required for their formulation would suggest) is this: given any specific complex symbol, constructed solely of the elements '(', ')' and '0' in the manner specified, it is possible to discover *in a finite number of steps*, (*a*) whether it is a pattern in the sense defined, (*b*) and, if so, the exact value of its rank. Furthermore, if two specific patterns are presented, a finite number of operations will suffice to determine which precedes the other unless they are identical.

Specialists will recognise in the 'patterns' Cantor's transfinite ordinals (or rather all the transfinite ordinals preceding Cantor's "first epsilon number") in disguise. Both Gentzen and Hilbert-Bernays use Cantor's original notation of 'ω's and positive integers compounded by successive exponentiation and multiplication by similar terms. The point of introducing the alternative notation of 'patterns' above is to demonstrate that Gentzen's methods are independent of the far-reaching assumptions needed to establish Cantor's theory in full generality. It is sufficient to define *ad hoc* symbols (as in nos. 1 to 8 above) without discussing their relations to more general types of symbols.

The fundamental property of 'patterns' upon which their usefulness to Gentzen depends is this: Suppose some specific pattern (of rank n, say) to be presented. And suppose we replace it by another pattern which precedes it, that in turn by a preceding pattern, and continue in this way, obtaining a progression of patterns each of which precedes the term which has been written upon it. *Then it will always be the case that such a progression will terminate in the pattern '0' after a finite number of terms.* (Once again, specialists will recognise a special case of the theorem that a descending progression of terms taken from a well-ordered series must be finite.)

The truth of this theorem is not quite obvious. But a little manipulation of sample patterns may convince the reader that the operation of replacing a pattern $(a_1a_2a_3 \ldots a_k)$, say, by a preceding pattern, can increase the total number of '0's in the pattern only by reducing the rank either of one of the 'a's or of one of the constituents of the 'a's. If the rank of no sub-pattern is reduced, the total number of '0's must diminish. Sooner or later then (though we cannot say exactly how much sooner or later) the rank of *each* sub-pattern, and thus the rank of the whole pattern, will be reduced. (The formal presentation of this argument would involve the process misleadingly called "transfinite induction".)

After these lengthy preliminaries, it is easy to outline Gentzen's proof. There are two stages: (i) We are shown how to correlate a unique pattern to each specific presented deduction in Z, (ii) a procedure is described by means of which each specific deduction in

Z is convertible into another deduction in Z having a preceding pattern correlated to it. In this way the question of consistency for any deduction in Z is reducible to the consistency of deductions having associated patterns of *unit* rank. And for the latter, the demonstration of consistency is trivial. Thus the consistency of the whole of Z has been demonstrated.

The novelty of Gentzen's proof (apart from details) arises from the use of transfinite ordinals or the equivalent " patterns " in order to arrange the deductions of Z in linear order. In so doing he undoubtedly breaks the restrictions observed by earlier formalist writers (he *must* do this, in order to escape the force of Gödel's theorem). The finitist method (as described in detail, for instance, in *G.M.*, 1, 20-44) provided that any existential statement about a number must contain a specified limit to the number of operations required to obtain that number (" zur Angabe eines Verfahrens gehört, dass für die Reihe der auszuführenden Handlungen eine bestimmte Grenze aufgewiesen wird ", *G.M.*, 1, 32). Thus in this interpretation of the finitist method, the statement (used above) that a progression of steadily preceding patterns must terminate in a finite number of steps, is admissible only if for each given (" als konkret vorliegend angenommen ") pattern a limit could be assigned for the number of operations required to convert it into a pattern of unit rank. But this is precisely what *cannot* be done ; for each pattern has an *infinite* number of preceding patterns, even though any *konkret vorliegend* progression of steadily preceding patterns must have a finite number of terms.

Is it then permissible to regard Gentzen's leading idea as ' finitist ' ? I believe anybody who tries to replace a specific pattern, constructed in the way described above, by a series of steadily preceding patterns, will receive a vivid impression of the elementary and intuitively convincing character of the type of argument involved. It may be noticed that a rather similar extension of the strictest finitist method is involved in the definition of Brouwer's free-choice sequences (*Wahlfolgen*). It seems incredible that such types of argument should involve the mathematical paradoxes. It seems, then, that Bernays is justified in blaming the previous lack of success of the formalist programme upon its excessive (*überspannt*) restrictions on permissible modes of research (*G.M.*, 2, vii). The formalists may now reasonably expect to be able to extend Gentzen's procedure to the theory of functions (*G.M.*, 2, 373, last paragraph). But if Gentzen is right in stating that the extension to systems more comprehensive than Z will require the use of symbols more complex than the above-defined ' patterns ' (the use of ordinals beyond the first epsilon number, *cf.* Gentzen, 43), such extension will call for even further relaxation of the earlier formalist restrictions.

Enough has been said to underline the importance to those interested in the philosophy of mathematics, of acquaintance with Gentzen's work and its comprehensive setting in the treatise of

Hilbert and Bernays. After a glance at these books, however, the philosopher without the requisite specialist training to feel at home in these orderly mazes of symbols, roman and gothic, may well ask what bearing these minute technicalities are likely to have upon his own more general interests. It is natural to draw comparisons in this respect with *Principia Mathematica*. There was a time when most logicians felt it a duty to read *P.M.* (or at least to perform the motions of reading). Of those whose sense of duty brings them with similar intentions to *G.M.*, few will get as much profit. The later book is less readable and less illuminating. (One misses, for instance, the excellent broad summaries of things to come which preceded the chapters of *P.M.*) Nevertheless, despite the deliberate exclusion of any issues which might be damned by being called philosophical, the newer techniques provide much which would deserve philosophical analysis. To take one example, the theory that the non-finitist symbols used in pure mathematics are to be interpreted as "ideal elements" (Gentzen, 16-18; *G.M.*, 1, 16) would provide a fruitful starting-point for a consideration of certain aspects of scientific method. If formalist theory is to make a contribution to general philosophy we shall need a book of more popular tendency bearing roughly the same relation to *G.M.* as Russell's *Introduction to Mathematical Philosophy* does to *P.M.* Perhaps some formalist who has a sneaking respect for philosophy will yet find time to supply the missing term in this bibliographical ratio.

M. BLACK.

The Concepts of the Calculus: A Critical and Historical Discussion of the Derivative and the Integral. By CARL B. BOYER. New York: Columbia University Press; and London: H. Milford, 1939. Pp. vi + 346. 18s. 6d.

WE are fortunate in having a mathematical historian who is a worthy successor to the late Prof. Florian Cajori. We have at last a wholly adequate account of the growth of the calculus. The scope of the present work is clearly indicated by its sub-title. Mr. Boyer has done more than give a clearly written historical record: he always keeps his matter alive by judicious criticism and by keeping before his readers those aspects of present-day mathematics towards which the historical conceptions of the calculus have evolved. Even though he refers to large numbers of minor mathematical figures of the past he always succeeds in distilling the point for which he brings them before us. This balance of judgment throughout makes history illuminating instead of, as is often the case, misleading. The book is fully documented and has an extensive bibliography. It is worth mentioning that Freshmen studying mathematics would probably find the first two chapters very helpful in understanding

the point of modern mathematical analysis—why, for instance, Cauchy's approach to limits is needed.

The problem of the calculus is traced from the difficulties that confronted Pythagoras and Zeno as regards infinite divisibility to their solution in the seventeenth and nineteenth centuries. Unlike many historians Mr. Boyer does not regard the history of the calculus as concluded when he has discussed its "founders", Newton and Leibniz, but as beginning; he thinks of the subject not merely as an algorithmic method but as this bound up with the mathematical analysis that constitutes its basis. By connecting the paradoxes of Zeno with the calculus we see the geometrical method of exhaustions of Greek times gradually lose its geometrical flavour, evolve to the method of fluxions with Newton, criteria for limits in the hands of Cauchy and Weierstrass, necessitate revision of the notions of irrational and infinite numbers due to Dedekind and Cantor, and finally require Mr. Bertrand Russell's definition of number to cover the arithmetical continuum. Thus Pythagoras first set the problem by being unable to show how $\sqrt{2}$ could be a number, and after 2500 years this was answered. The calculus is thus seen as a phase in the treatment of the question: What is number? It must not be supposed, however, that the calculus is presented purely as a development in mathematical analysis, for its purpose in solving questions of motion and geometrical problems are also stressed. Mr. Boyer gives some interesting historical methods of finding areas, which on account of their restricted application show the need for the calculus.

We may summarise the development of the arithmetisation of mathematics as consisting of two stages: (1) the transition from the Greek method of exhaustions, which was valid but insufficiently wide in scope, to the method of infinitesimals, which was invalid but fertile; and (2) the transition from this calculus to mathematical analysis, which like the method of exhaustions is valid, but which like that of infinitesimals has considerable potency.

In discussing the growth of the concepts of the calculus among the Greeks Mr. Boyer describes the difference between the process of exhaustions and that of the calculus: he points out that in obtaining an area by exhaustions the area is never literally exhausted —the limit is not attained—while in doing so by the calculus the area is literally exhausted. This is an illuminating distinction, which enables us to see how far the Greeks anticipated the calculus and wherein they fell short of it; but at the same time the author makes a somewhat curious mistake: he writes (p. 34) that in proceeding to the limit we literally pass over an infinity of terms. This is a strange way of putting the matter, for it is precisely what we do not do. On the contrary we frame a definition of "limit" so that the limit is attained; the whole point is to *define* the passage over an infinity of terms in such a way as to avoid the notion of an infinite aggregate added up literally one by one. It would follow,

moreover, that it is not correct to talk of the modern process of integration as *exhausting* an area: it does so in comparison with the Greek method, or, at any rate, to say so gives quite a good way of distinguishing the two methods; but from the standpoint of modern rigour an area is neither incompletely exhausted nor completely so, for it is not attained bit by bit as the word "exhaust" implies. A good way remains, however, of describing the difference between the two methods, which is that the Greeks had to know the answer in advance—by some good technique of guessing—before applying exhaustions for purposes of rigorous proof, whereas the method of the calculus both obtains and justifies the answer.

The transition from the one method to the other is noteworthy for, among other reasons, the following. The Greeks concentrated on rigour and *it was rigour that hampered the development of the calculus*. What progress was made by Archimedes arose when he moved away from Greek tradition. The rise of infinitesimals over a long period, loosely used, eventually led to the method of fluxions. It is true that, in their original form with Newton, they were nothing more than a good technique of guessing, and had to be justified by rigorous analysis, which took 90 years to come into being from the time when the technique was first recognised to be illogical, and 140 years to reach maturity. None the less to discover the method in the first instance demanded that rigour should be relaxed—mathematicians had to know what they were to be rigorous about before becoming rigorous about it.

From the history of the infinitesimal period we learn that discovery is not the work of one man only. Mr. Boyer repeatedly takes pains to point out that the calculus was discovered by Newton in a background pervaded with infinitesimals and the main aspects of what became the method. Fermat and Barrow all but discovered the calculus, as afterwards Bolzano nearly achieved Cauchy's work on limits. Accordingly, when we talk of Newton as the "founder of the calculus", we mean that an important *stage* in the development of mathematics took place. Humanly speaking, we are apt to divide up a period into striking stages, specially marking the initial and final ones. This is not, however, unfair to reputations, for probably what requires the greatest originality is inauguration of a new idea and rounding it off. But if the author claims that we cannot attribute a discovery to one man, he equally warns us against reading modern discoveries into the work of the ancients and supposing that they had achieved the modern standpoint.

Now we come to Newton's method of fluxions. It is usual to suppose that this was based either on infinitesimals or on limits, but Mr. Boyer asserts (p. 200) that three alternatives have to be considered. "Newton thus offered in the *Principia* three modes of interpretation of the new analysis; that in terms of infinitesimals (used in his *De analysi*, his first work); that in terms of prime and ultimate ratios or limits (given particularly in *De quadratura*, and

the view which he seems to have considered most rigorous); and that in terms of fluxions (given in his *Methodus fluxionum*, and the one which appears to have appealed most strongly to his imagination)." The author does not distinguish the third of these very clearly from the others, but presumably it refers to the geometrical treatment of tangents, which however could be based either on infinitesimals or on limits. If we ask what was Newton's view we find he regarded them as essentially equivalent—he did not fully realise, though he seemed to feel it dimly, that one was invalid and that the other required a tremendous amount of justification. Whichever we attribute to Newton, the method of infinitesimals, because it was invalid, and that of limits, because it was not yet supplied with a valid basis, were equally open to logical criticism. This was provided by a philosopher. Berkeley delivered a decisive attack in *The Analyst* (1734) and passed as perfect criticism on the method of fluxions as could be wished. It does not appear to be generally recognised among philosophers how much the development of pure mathematics for the next 90 years owed to Berkeley's able destructive criticism; but owing to it mathematicians both in Great Britain and on the Continent concentrated once more on rigour, which led to the crowning achievements of Cauchy, Weierstrass and Dedekind. Berkeley criticised the contradictory nature of infinitesimals, which were used as if they had the property of being both zero and finite—"zero-increments", we may call them: finite increments were introduced in order to calculate a fluxion, but the last step in the calculation consisted in treating these as zero, and once these quantities departed there remained the fluxion. He might have cited Zeno's aphorism, directed against Pythagoras (p. 23): "That which being added to another does not make it greater, and being taken away from another does not make it less, is nothing". But, more dramatically, he dubbed fluxions "ghosts of departed quantities". Mr. Boyer might have referred to Cajori's comment that they were "the liveliest and most productive ghosts ever placed upon the stage of intellectual history": illogical they were, but more powerful they could scarcely have been.

When the controversy passed out of Berkeley's hands, a dispute arose about whether or not the incrementary ratio attained its limit. Robins, who gave the best exposition of fluxions of his time, held that they did not; it is therefore clear that he went astray, as has been pointed out by Cajori. Mr. Boyer regards Cajori's criticism as quite beside the point (pp. 231-232 and 287-288), because (*a*) the discussion implies something intuitive and therefore irrelevant, and (*b*) the question of attaining a limit depends upon our definition of "a limit"—thus if the limit of an infinite sequence is defined as identical with the sequence itself, no argument can arise. But while this is just, it appears to miss a vital point: it gives the impression that mathematicians wasted their time in such discussions, whereas in fact they were led to discover a definition of "limits"

that would satisfactorily close the discussion. Mr. Boyer's remarks on the intuitive element involved are pertinent. We must not think of the sequence as *moving*, which is a spatial metaphor, towards its limit, because the limit concept involves only a *static* state of affairs: "The single question amounts really to two: first, does the variable $f(x)$ have a limit L for the value a of x. Secondly, is this limit L the value of the function for the value a of x. If $f(a) = L$, then one can say that the limit of the variable for the value of x in question is the value of the variable for this value of x, but not that (x) *reaches* $f(a)$ or L, for this latter statement has no meaning" (pp. 287-288). If we are to revise Cajori's criticism of Robins in the light of Mr. Boyer's improved language, we should have to say that, according to Robins (though if he had been explicitly faced with the question he might have altered his answer), $f(a)$ cannot be equal to L.

Mathematical analysis has arisen through the gradual elimination of the geometrical and intuitive. The author strongly emphasises that the intuitive has no place in the notion of a limit or that of continuity. As regards the problems of Zeno that the calculus solves, he maintains (a) that no solution other than the mathematical one is possible, (b) that it is sufficient, and (c) that the failure of this solution to satisfy our intuition must be deemed irrelevant. Thus the question of how Achilles overtakes the tortoise is solved by a valid basis for infinite series, and we cannot help it if we cannot envisage the terms of an infinite series or its sum. But this surely is hardly satisfactory, for no amount of acquaintance with the convergence of series alters the plausibility of Zeno's arguments. All that the mathematics does is, after all, to show that Achilles is behaving validly in overtaking the tortoise, and, since we know by observation that he can do so, it is good to know that his action is legitimate; but we have yet to understand wherein Zeno's argument is invalid. We might attempt to explain this on the grounds that *having a speed*, *being at a place*, and *taking a certain length of time* are conceptions so familiar that we find it beyond us to believe they have no meaning except what can be given to them scientifically. A speed in practice is the average speed calculated over as short a stretch as possible; being at a place means not being outside a certain definite and sensibly observable contour; taking a certain time means not taking longer than a certain definite and sensibly observable interval. These may be definable for mathematical purposes by Prof. Whitehead's principle of extensive abstraction, but in practice, one regards them from an unsophisticated statistical angle. None the less we are apt to have it both ways: as well as these averaging notions that we use in practice, we usually, on conscious reflexion, think of a point as a very small thing of no size, a place similarly, a time as a short moment of no duration, a speed at any instant as being the speed at a moment so short that time does not flow—all which have the same sort of contradictory

flavour as infinitesimals regarded as finite increments of no size. *Now when Achilles gets near the tortoise, we can only endow him with a speed or a further distance to run if we first allow him to run, passing the tortoise in the process, and then calculate his speed and distance on the basis of our observed measurements:* that is to say, when near the tortoise, a speed that would operate in a time sufficiently short to prevent him catching the tortoise could not be estimated, nor could the distance travelled over. If this is correct, the plausibility of Zeno's arguments depends upon our passing from our ordinary averaging notions of space, time and speed, to absolute ones, which apparently are the most convincing to uncritical conscious reflexion, but which have no correspondence with fact.

With the weeding out of the intuitive, Mr. Boyer has to explain how an abstract analytical system can serve as a descriptive language for the physical world. To this his answer is that, though pure mathematics passes from the qualitative to the quantitative, and again to the ordinal, it never loses contact with the physical world, since it was from certain quantitative features of our experience that mathematics evolved. " Thus only may be fully appreciated the twofold aspect of mathematics: as the language of a descriptive interpretation of the relationships discovered in natural phenomena, and as a syllogistic elaboration of arbitrary premises " (p. 309). It remains something of a problem, however, to explain why the physical world conforms to what the author so eloquently calls " well-behaved " functions (p. 246), *i.e.* those that are continuous and have a derivative, or those that converge.

The history of the subject makes us realise very forcefully how difficult it was. The lack of good expositions alone proves this. By a curious coincidence the first reasonably adequate exposition of the calculus was published in 1787, exactly one hundred years after the publication of Newton's *Principia*, by L'Huilier—the Berlin Academy had hit on the expedient of offering a prize for the best exposition of a clear and precise theory of mathematical infinity (pp. 254-255). That the calculus still provides its difficulties appears from a discussion of differentials that took place as recently as 1934-36, in which a certain mathematician would seem not to have appreciated Cauchy's work, defining them in terms of derivatives (p. 276).

It would be a pity to conclude without recording Mr. Boyer's reference to d'Alembert's mathematical background as " geometrical ideology " (p. 249) !

J. O. WISDOM.

VIII.—NEW BOOKS.

Correspondance entre Harald Høffding et Émile Meyerson. Publiée par Frithiof Brandt, Hans Høffding et Jean Adigard des Gautries. Copenhagen: Einar Munksgaard, 1939. Pp. xiv, 202. Price Cr. 7.

Høffding and Meyerson never met. The correspondence was started by Meyerson in 1918, when Høffding was 75 and Meyerson 59, and continued until the death of Høffding in 1931. Meyerson himself died two years later. The period was one of steady philosophical activity for both. In it Høffding wrote his well-known studies on *Totality*, on *Relation*, and on *Analogy*, and the book which he himself translated into German with the title *Erkenntnistheorie und Lebensauffassung*. Meyerson was greatly interested in these works, and arranged for French translations, himself supervising the translation of the studies on *Totality* and on *Relation*. His own books *La Déduction Relativiste* and *Du Cheminement de la Pensée* were written during this period.

The letters reflect all this activity. The correspondents write of the progress of their work, and explain the main ideas they are endeavouring to develop; and there are interesting sidelights on their relations with publishers, and particularly on the affairs of Félix Alcan.

Meyerson had a high regard for Høffding. He addresses him always as Cher et vénéré maître, and confesses that "vous êtes en ce moment-ci pour moi . . . l'arbitre suprême dans toute la domaine philosophique" (p. 10, Sept., 1921). Perhaps this intensity of admiration is a disadvantage from certain points of view, for Meyerson was anxious to find himself in agreement with Høffding, and on occasion met Høffding's criticisms rather with explanations tending to show how little difference there was between their points of view, than with the firmness he could display so well in other circumstances. In Høffding's view there was not the fundamental difference which Meyerson insisted on between the concept of law and the concept of thing or substance. In a letter of 29th May, 1922, Høffding notes three differences between his view and Meyerson's:—

(i) Høffding thought that for science the notion of activity was more fundamental than that of substance, and looked forward to the vanishing of all traces of ontology from science. He stressed, not identity, but continuity. "Tout ce que nous pouvons savoir sur la 'substance', c'est la loi de son action" (p. 43). He spoke of a "dynamic" as opposed to a "static" concept of truth.

(ii) Again Høffding felt that along the path taken by Meyerson the crucial point of difficulty in the problem of knowledge tended to be left out of sight. For Meyerson the passage from quality to quantity (as in the scientific theory of colours, for example) once taken, was no longer in the centre of interest; the important problems lay for him in the next step of science, the attempt to find ultimate elements, which should be identical through change and yet explain change; and his interest lay in the history of this attempt, in its perpetual failure and in what was to be learned

therefrom for the building up of a general theory of human knowledge. Whereas Høffding concentrated attention rather on the fact that there are qualities, and on the radical disparity between quality and quantity. It is only by analogy, he thought, that a series of qualities can be "explained" by means of a series of quantities; and it seemed to him important to analyse the concept of analogy and to see the way in which it was used by the mind in the building up of knowledge.

(iii) Thirdly—and I think this point is closely connected with the second—Meyerson held that science is compelled to abstract from the particular detail of individual things, while Høffding thought that it is possible to arrive at a more and more complete knowledge of the individual, "en étudiant comment des lois différentes coopèrent à la naissance et au développement d'un phénomène individuel (inorganique ou organique, psychologique ou sociologique)" (p. 44).

Høffding sums up these differences by noting that while Meyerson came to philosophy through the history of science, he himself came to it through psychology and history.

Høffding's criticism of Comte (p. 9) and his book on *Erkenntnistheorie und Lebensauffassung* show the same tendency toward the individual and the concrete. In his book, he tells Meyerson (May, 1925, p. 95) "je m'occupe spécialement du contraste de l'idéal scientifique que l'Epistémologie nous décrit et des totalités vitales et psychiques". He is thinking not only of the contrast (*e.g.*) between the field studies of the biologist on the one hand and experimental physiology on the other, or between the facts of concrete psychology and the attempts at a mechanistic explanation of these facts. There is a parallel contrast in a wider field. In all of us, he says, there develop spontaneously "conceptions de la vie et du monde"—outlooks on life and on the world, scientific, metaphysical, religious—which have the characteristic of being totalities; and there is a sharp opposition between them and the rationalistic analyses of them which are furnished by psychological and historical studies. He speaks of them as "syntheses": but by this he does not mean that they have been built up out of parts first consciously apprehended in separation. He describes these syntheses as "anthropomorphisms" (pp. 99-100), which analysis shows as arising under specific conditions, according to certain general laws of the human mind; and he suggests that metaphysics and religion are particular forms, belonging to a particular stage of human development, of "une poésie de la vie, qui, j'espère, se développera dans l'avenir plus librement que dans le passé et nous donnera le fruit de ce que des hommes ont véritablement vécu, de leurs expériences les plus intimes" (p. 100).

In his reply to Høffding's account of the differences between them Meyerson gives some interesting details of his own development (p. 102 f.). Later on he gives an account of the effect on him of Kroman's book, translated into German under the title *Unsere Naturerkenntnis* (1883), which he came across in 1890 when he was working at the *Agence Havas* (p. 141).

The letters touch on a variety of topics. We may mention, *e.g.*, the notes on the relations between Schelling and Hegel (a matter Meyerson had discussed at length in *De l'Explication*), and the brief discussion on the influence of Mach on German thinkers. Høffding thought Meyerson over-estimated this, and considered the influence of the Marburg school to be greater. Meyerson agreed, so far as the philosophers were concerned, but thought that it was different with the scientists. His note on the matter is characteristic (pp. 113-115).

A good deal of the correspondence is taken up with personal details. Meyerson had a long struggle against the disease to which he finally succumbed, but in spite of pain and sleeplessness he never lost heart, and continued to widen his philosophic studies and to deepen his knowledge of contemporary thought. The letters are a precious though in part a melancholy souvenir of a very gracious personality. Høffding on his side suffered through the death of his eldest son and through the illness from 1928 and premature death in 1930 of his young wife, whose cheerfulness and courage had meant so much to him in his old age. He died nearly a year later.

The volume is beautifully produced. M. Hans Høffding, who had made a transcript of his father's letters in preparation for the edition, died in 1937, and his co-editors completed the work. All admirers of Høffding and of Meyerson are under a debt of gratitude to them for their loving care.

L. J. RUSSELL.

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C. E. M. Joad, *Philosophy for Our Times*, London, T. Nelson & Sons Ltd., 1940, pp. vi + 367, 7s. 6d.

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A. H. Dakin, *Man the Measure : An Essay on Humanism as Religion*, Princeton, Princeton University Press ; London, H. Milford, 1939, pp. xiii + 284, $3.00.

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G. Murray, *Stoic, Christian and Humanist*, London, C. A. Watts & Co. Ltd.; G. Allen & Unwin Ltd., 1940, pp. 189, 5s.

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G. R. Morrow, *Plato's Law of Slavery in its Relation to Greek Law*, Urbana, The University of Illinois, 1939, pp. 140, $1.50.

G. Percival, *Aristotle on Friendship: Being an expanded translation of Nicomachean Ethics, Bks. VIII and IX*, Cambridge, University Press, 1940, pp. xxxix + 151, 10s. 6d.

G. G. Coulton, *Studies in Medieval Thought* (Discussion Books No. 65), London, T. Nelson & Sons Ltd., 1940, pp. 229, 2s. 6d.

H. H. Joachim, *Spinoza's Tractatus de Intellectus Emendatione: A Commentary*, Oxford, Clarendon Press, 1940, pp. xvi + 231, 15s.

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J. Maier, *On Hegel's Critique of Kant*, New York, Columbia University Press; London, H. Milford, 1939, pp. viii + 108, 9s. 6d.

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R. S. Woodworth, *Psychological Issues*, New York, Columbia University Press; London, H. Milford, 1939, pp. x + 421, 22s.

M. McTaggart, *Man, Mind, and Psychology*, London, Watts & Co., pp. 83, 3s. 6d.

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G. Dworetzki, *Le Test de Rorschach et l'Évolution de la Perception*, Geneva, Librairie Naville et Cie, 1939, pp. 234-396, 6 fr.

J.-E. Marcault et T. Brosse, *L'Éducation de Demain*, Paris, F. Alcan, 1939, pp. xi + 305, 40 frs.

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L. Blake, *The Last Year of War and After*, London, A. Dakers Ltd., 1940, pp. 212, 3s. 6d.

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F. Oates, *A Philosophic Study of the Forces of Rest and Motion*, pp. 20, £2 12s. 6d.

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T. Tiplady, *Hymns for the Times*, London, Epworth Press, 1939, pp. 36, 2d.

Journal of the History of Ideas, Vol. I, No. 1, edited by A. O. Lovejoy, New York, Journal of the History of Ideas, Inc., 1940, pp. 128, Subsc. rates, domestic $4 per annum, foreign $4.50 per annum.

IX.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF PHILOSOPHY, XXXVI (1939), 15. **C. A. Strong.** *The Sensori-Motor Theory of Awareness.* [Discusses Prof. Montague's criticisms of "A creed for sceptics", and gives a further account of certain matters dealt with in that book. Explains also why he disagrees with Mr. Santayana's view that the datum is an "essence". Claims that his analysis of the real implies that human action is determinate, and appears to see no difficulty in reconciling choice with determinism.] **Arthur F. Bentley.** *Postulation for Behavioral Inquiry.* [Asserts five "postulates for 'situational' inquiry into behaviors". These "are *not* items of belief. They are tools to work with." At the same time they are "meant to be taken literally, but for taking them literally lip-service is not enough. Their programme is action. Observation—widened and sharpened observation—is the heart of them." The first postulate is "Organism and Object. In behavioral inquiry, organic and non-organic participations cover the field without third participant." Says it is difficult to eject third participants such as minds. Gives no indication of how the postulates are to be used in sociological investigation.] 16. **Daniel Cory.** *The Private Field of Immediate Experience.* [Advances two arguments to show that "all the immediate furniture of the mind is lodged in a private field of perception". Argues firstly from dreams that we have "a capacity for imagining, or speciously projecting, objects beyond the picture of our own bodies". Suggests that "in waking perception as well as in our dream-life, what we are immediately acquainted with are . . . private sensuous constructions which we spontaneously take to be physical objects. . . ." His second argument is based on the causal conditions of perception.] *Comments and Criticisms.* **Isaac Husik—Sir William David Ross.** *The Authenticity of Aristotle's Categories.* 17. **Sterling P. Lamprecht.** *Historiography of Philosophy.* [Attacks the view that "the historian should stick to the facts and not enter into appraisals of meaning because meanings are merely subjective". Says that "meanings are hypotheses to be tested by reference to future situations". "Any history has as many meanings as it has uses to which it may legitimately be put. . . . . legitimacy of meaning is utility of subject-matter in constructive reference to the future situations relatively to which the historical enterprise is undertaken." In regard to philosophy says "To take two analyses as opposed philosophies and to stage a debate between them may often be to miss the point of them both".] **John Herman Randall, Jr.** *On Understanding the History of Philosophy.* [Accepts "Prof. Lamprecht's analysis of the historian's enterprise as a foundation on which to build". Discusses the difficulty raised by Harry Elmer Barnes, that the historian has both to "trace the genesis of contemporary culture and institutions" and "to reconstruct as a totality the civilisations of the leading eras in the past". The article is about the philosophy of history rather than the history of philosophy.] 18. **A. O. Lovejoy.** *Present Standpoints and Past History.* [(*a*) "It is not factually

true that historical inquiries are necessarily or commonly motivated by present non-historical problems; and . . . they should not be determined by or restricted to such problems." (*b*) ". . . the consequences sometimes drawn with respect to historiography from the presenticentric predicament are inadmissible. If you are willing to accept the fundamental postulates of science and common sense, you can no longer deduce solely from the *general* fact of the presenticentric predicament any special conclusions with regard to historical inquiry and knowledge."] **Herbert W. Schneider.** *A note on Dewey's Theory of Valuation.* ["Admitting all that Dewey has to say concerning valuable value-judgments, I merely wish to add that there may be value-gestures or propositions that serve no useful function except to close an act of thought. If this is true, we should be obliged to admit that not all value-propositions imply need, desire, or "active" interest *on the part of the person making the proposition* nor "aversion to an existing situation" nor an "end-in-view".] 19. **John R. Reid.** *The Dilemma of Definitions.* [Criticises the bewildering variety of senses of "definition" used by philosophers—especially "real" definition. ". . . if we try to use words as other men do, we quickly find . . . that these words are used in so many different senses . . . that there is little hope of being understood unless we go into endless qualifications. But if we do this, our writing becomes so involved that few persons can follow the verbal labyrinth of our thought."] **A. Cornelius Benjamin.** *What is Empirical Philosophy?* ["An empiricist is not merely an empiricist, but an empiricist of a certain scope (degree of liberality, generosity, open-mindedness, inclusiveness of outlook). The extremes of these dimensions represent, at the one end, the world view and the intellectual attitude of the positivist, who limits the factual to the here and now, and considers all else as construction, and at the other end, the world view and intellectual attitude of the anti-positivist, who finds the world composed of a number of things, many of them confused and obscure, but all of them giving promise of yielding to clarification, and who takes in this extremely diversified world a genuinely speculative interest."]

Revue néo-scolastique de Philosophie. Tome 42. (Deuxième série No. 64). Nov. 1939 (with bibliographical supplement). **A. Marc.** *Le point de départ d'une psychologie rationelle.* [A "rational," as distinguished from an empirical or experimental, psychology is possible on the condition that we can identify some type of act which is specifically human as a datum of experience, and can by analysis of it penetrate behind the act to the inner state or activity of which it is the expression. Now such a datum is provided by the act of speech (significant speech), which is common to and peculiar to mankind. We see with particular clearness from the histories of blind-deaf-mutes, such as Helen Keller and others, that it is the act of using a gesture as an arbitrary sign of something other than itself which rapidly and almost suddenly raises the subject from the level of the brute to that of humanity. Such a use of significant signs is found to be totally absent from what has been called the "language" of animals. The writer proceeds to illustrate the way in which this employment of gestures as arbitrary signs of something other than themselves, entails recognition of the reality of both the object signified and the subject employing the sign, and the real distinction between the two. The essay then proceeds to indicate points of contact between the results of impartial psychological analysis of the use of language as a system of symbols and the mediæval theories of *suppositio* and *significatio*

reached by logical analysis.] **A. Fauvelle.** *La psychologie scientifique est-elle systématique?* [A review of the history of "experimental" psychology and an examination of its methods lead alike to the conclusion that there is not and, in the present state of the subject, there cannot be any satisfactory general system of scientific psychology. Any general scheme based on the very partial data available must be lacking in exactitude and certainty, and can have no scientific worth or use. Experimental psychology fails to grasp "psychical phenomena" in their full reality, since this eludes scientific measurement. Hence the legitimacy in psychological inquiry of less exact, but more far-reaching and more fruitful, observations. Hence, too, the legitimacy of "non-scientific" psychological schemes (Bergson, Dewey, etc.) which aim at apprehending either psychical life or some of its leading manifestations *dans leur aspects d'ensemble.*] **L. Noël.** *Encore l' "illationisme" du Cardinal Mercier.* [A sharply polemical reply to M. Gilson's attempts to show that the "realism" of Cardinal Mercier is only "indirect," resting upon inference.] **F. van Steenberghen.** *La littérature Thomiste récente.* Book Notices, Chroniques, *Répertoire bibliographique.*

X.—NOTES.

THE EDITOR, "MIND",

Note on "Did Berkeley Misunderstand Locke?" (Jan. no., p. 52.)

Mr. Barnes' careful discussion of this interesting little problem succeeds, I think, as an *argumentum ad* Mr. Jackson; but the correctness of confining the reference of *Principles*, 9-15, to Locke is open to question. If Berkeley were thinking solely of Locke, he would probably have named Locke, or the *Essay*, as he does in the *Theory of Vision*, 125, 130, 132, and in the *Principles, Intr.*, 11, 13. I suggest that he has Malebranche in view, as well as Locke. Berkeley knew the *Recherche* nearly as thoroughly as he knew the *Essay*, and when he was debating the alleged distinction between primary and secondary qualities, he could not have ignored Malebranche, whose doctrine of 'modifications of the soul' carries the distinction to a point well beyond anything contemplated by Locke.

The phrasing and the argument of these sections support my contention; for while the terms *primary, secondary*, and *original* are exclusively Lockeian, the triad *extension, figure, motion*, is specifically Malebranchian, and this triad comes more frequently and more naturally to Berkeley's lips than do any of Locke's varying lists of primaries. Again, the relativity arguments of section 14 echo the writings of both these 'modern philosophers'; the body warm to one hand and cold to another is reminiscent of Locke, II, 8, 21; and 'the sweetness is changed into bitter, as in the case of a fever', is close to Malebranche's 'The same person who thinks it pleasant when he's in health, finds it bitter in a fever' (*Recherche*, I, 13; Taylor's tr., 1694).

Berkeley's double aim would explain any slight indefiniteness which may appear in his presentation of the doctrine; he is not concerned to refute Locke in detail; but, following up perhaps the hints given by Bayle (*Articles, Zeno, Pyrrho*), he attacks all who base the existence of matter on the alleged distinction between two types of sensibles.

This explanation is specially appropriate in the case of the extract from section 10, which Mr. Barnes regards as somewhat difficult to defend; for the passage contains the word 'occasioned', a *vox propria* in Malebranche's doctrine; the word occurs more than once in the *Recherche*, Book I, and does not occur at all in the famous chapter of the *Essay* (Book II, c. 8) where Locke expounds his theory of the qualities. It should also be noted that Berkeley did not write the Lockeian phrase 'and suchlike secondary qualities' in his first edition (1710), but added it in his second edition (1734, *v.* T. E. Jessop's ed., p. 32).

A. A. LUCE.

Ryslaw,
Bushy Park Road, Dublin,
26*th January*, 1940.

STUDENTSHIP IN PSYCHICAL RESEARCH.

Trinity College, Cambridge, has established a Studentship for the study of Psychical Research out of a bequest left to the College for that purpose by Mr. Frank Duerdin Perrott as a memorial to F. W. H. Myers.

The Electors to the Perrott Studentship are prepared to receive applications from candidates.

Psychical Research is defined, for this purpose, as 'the investigation of mental or physical phenomena which seem *prima facie* to suggest (*a*) the existence of supernormal powers of cognition or action in human beings in their present life, or (*b*) the persistence of the human mind after bodily death'.

The Studentship is open to any person who shall have completed his or her twenty-first year at the time when the election takes place. A Student may be re-elected once, but not more than once.

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating some problem in Psychical Research.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake. The emolument will, in general, be paid half-yearly, and the first instalment will be paid on the quarter-day on which the tenure of the Studentship begins.

The Student shall, during the tenure of his Studentship, pursue to the satisfaction of the Electors the course of research proposed by him in his application; provided that such course may be altered with the consent of the Electors. If the Electors shall report to the Council of Trinity College, Cambridge, that he is failing to pursue his course of research with due diligence, the Council may, if they think fit, deprive him of his Studentship.

The Student shall not, during the tenure of his Studentship, follow any business or profession or engage in educational or other work which in the opinion of the Electors would interfere with his course of research.

Applications from candidates should be sent to Prof. C. D. Broad, Trinity College, Cambridge, before 6th May, 1940. In making his application a candidate should state his qualifications and claims, and his proposed course of research; he may also submit any work which he has written, published or unpublished. No testimonials are required from candidates who are graduates of Cambridge University or women students on whom a title of a degree has been conferred by that University. Other candidates must submit the names of three referees, and the Electors will not award the Studentship to any such candidate until they have had a personal interview with him.

The election to the Studentship will take place in the Easter Term of this year, and, if a candidate be elected, his tenure will begin at Michaelmas following the election.

ERRATUM.

In the first sentence of the first article in the January number of MIND, Prof. Stout quotes Prof. Laird as having given 1696 as the date of the publication of Hobbes' *De Corpore*. This is an error: Prof. Laird in the passage quoted (*Proceedings of the British Academy*, Vol. XXIV., p. 389) gave the correct date, which is 1655. In 1696 Hobbes had already been dead 17 years.

MIND ASSOCIATION.

Those who wish to join the Association should communicate with the Hon. Secretary, Mr. W. C. KNEALE, Exeter College, Oxford; or with the Hon. Treasurer, Mr. J. I. MCKIE, Brasenose College, Oxford, to whom the yearly subscription of sixteen shillings should be paid. Cheques should be made payable to the Mind Association, Westminster Bank, Oxford. Members may pay a Life Composition of £16 instead of the annual subscription.

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